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THE OBSERVATORY,

A
MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

E. W. MAUNDER, F.R.A.S.,
A. M. W. DOWNING, M.A., F.R.A.S.,
AND T. LEWIS, F.R.A.S.

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INDEX TO VOL. VIII.

Aberration, Theory of, 28.
Airy's 'Gravitation,' 92.
Aldebaran, Occultation of, 128.
——, ——, in sixth century, 86.
——, Old occultations of, 55.
Almucantar, The, 387.
Andromeda, New Star in the Great Nebula in, 321, 330–335, 386, 441.
——, Small Nebula in, 386.
——, Spectrum of the great Nebula in, 385.
Anonymous Stars of Yarnall's Catalogue, 387.
Apparent Enlargement of Celestial Objects near the Horizon, 130, 439.
Aquilæ, β, Brightness of, 271.
Astronomical Memoranda, 34, 98, 143, 191, 223, 254, 286, 319, 351, 402.
Astronomical Prizes of the Academy of Sciences, 182.
—— ——, Warner, 90.
Astronomische Gesellschaft, 253, 353.
Astronomy at the American Association, 393.
Atmospheric Absorption, Amount of, 57.
Australia, Determination of Fundamental Meridian for, 59.

B*ackhouse*, Parallax-hunting, 89.
Backlund, Ephemeris of Encke's Comet, 33, 64.
Ball's 'Story of the Heavens,' 370.
Barnard, Changes in Length of Red Spot on Jupiter, 244.
——, Comet 1884 *b*, 123, 161.
——, Comet II. 1867 (Tempel's), 210.
——, Comet Brooks (1885 *b*) and Transit of the same over a star, 362, 363.
——, Dark Transit of Jupiter's Fourth Satellite, 211.
——, Encke's Comet, 122.
——, Nebulæ, 210.
——, ——, New, 123.
——, Pons-Brooks Comet, 325.
——, Spots on Jupiter, 211.
——, Two Small Red Stars, 124.
Basle, Society for popular Astronomy at, 61.
Baxendell, Maxima and Minima of Variable Stars in 1884, 170.
——, Variable Star U Cygni, 20.
Baxendell, J., jun., Maxima of Variable Stars in 1884, 172.
Bayer's Lettering of the Constellations, 245, 377.
Bliss, Astronomer Royal, 86.
Bonner Durchmusterung, Distribution of Stars of, 58.
Bordeaux Observatory, Latitude of, 439.
Brewster's Line Y, 61.
Brown's 'Phainomena of Aratos,' 397.
Brussels, Royal Observatory, 253.

Cambridge Observatory, 314.
Cape Catalogue for 1880, 176.
Capron, Hyginus N, 294.

29485.

Catalogue of Stars for 1875 of Harvard College Observatory, 137, 252.
Centauri, α, Recent Measures of, 382.
Cephei, U, 178.
Christiania, Astronomy at, 251.
Chromosphere in 1884, 53.
Chronograph, A Printing, 442.
Cincinnati Observatory, 311.
Comet Barnard (1884*b*), 123, 161.
—— —— ——, Transit of, over stars, 30.
—— Wolf (1884 *c*), Ephemeris for, 33.
—— —— ——, Transit of, over two stars, 94.
—— 1885 *a*, Elements and Ephemeris for, 285.
—— of 1618, 126, 217.
—— of 1717 and 1718, 271.
——, Biela's & Denning's (1881 *f*), 257, 306, 399.
—— Brooks, Elements and Ephemeris for, 348.
——, Encke's, 30, 33, 61, 64, 122.
——, Halley's, of 1717, 247.
—— Tempel (II. 1867), 210.
—— —— ——, Alleged Discovery of, 221.
—— —— ——, Elements and Ephemeris for, 140.
——, Tempel's, of 1866 and the November Meteors, 386.
——, Tuttle's, 284, 311, 347.
Commensurability of Motions, 439.
Committee of Stellar Magnitudes, Third Report of, 436.
Corder, Winter Meteor-Showers, 88.
Cordoba Zone Catalogue, The, 18, 311.
Cornelius Gemma and Star of 1572, 431.
Corona, Solar, 153, 183.
——, Photographing the, 218, 277, 376.

Dearborn Observatory, 135, 441.
Denning, Biela's Comet, Meteor Shower of Nov. 27, 1872, and Comet 1881 *f*, 257.
——, Defining Powers of Telescopes, 205, 340.
——, Ephemeris of Red and White Spots on Jupiter, 31, 63, 95, 141, 190.
——, Halley's Comet of 1717, 247.
——, Jupiter and the relative Powers of Telescopes, 76, 300.
——, Jupiter's First Satellite occulting its Shadow, 159.
——, Partial Obscuration of Red Spot on Jupiter, 214.
——, Red Spot on Jupiter, 337, 383.
——, Transits and Nomenclature of Jupiter's Satellites, 159.
——, Twilight Fireballs from Musca, 24.
Domes, Floating, 253.
——, Rotation of, 138, 216.
Doolittle's 'Practical Astronomy,' 438.
Double Stars, A Cause of systematic Error in Measures of, 89.
—— ——, Observations of, 391.
Downing, Cordoba Zone Catalogue, 18.
——, Solar and Stellar Parallax, 212.
——, Souchon's 'Traité de l'Astronomie pratique,' 329.
Draconis, α, Variability of, 377.
Draper, on Carbon Bisulphide Prisms, 183.
Dreyer, Supposed Proper Motion of the Nebula *h* 705, 175.
Dun-Echt Observatory Publications, 342.
Dunér's 'Third-type Spectra,' 383.

Earth, Density of, 221.
Eclipse at death of Nerva, Alleged, 30.
—— Expedition to Caroline Island, May 6, 1883, 133.

Eclipse, Total Lunar, of Oct. 4, 1884, 131.
——, Total Solar, of May 12, 1706, 270.
——, Total Solar, March 16, 1885, 178.
Editorial Notes, 30, 62, 63, 95, 140, 189, 253, 285, 314, 399, 400.
Egbert, Comet 1884 *b* (Barnard), 161.
Encke's Comet, 30, 33, 61, 64, 122.
Engelmann's Observations of Double Stars, 391.
Equatoreal Coudé, 27.
—— Telescopes, The Adjustment of, 9, 43.
Eridani, o^2, Parallax of, 345.

Faye's Cosmogonic Theory, 184, 347.
Fireballs from Musca, 24.
Flamsteed's Stars, Notes on some of, 265.
Forbes, Prime Meridian Conference, 23.
Frisby, Proper Motion of Lalande 16616, 293.

G*autier*, *E.*, Est-il opportun de changer le Mode de compter le Jour en Astronomie ?, 420.
Gautier, *R.*, Elements and Ephemeris of Tempel's (II. 1867) Comet for 1885, 140.
Gill, Cape Catalogue for 1880, 176.
Gore, Notes on some of Flamsteed's Stars, 265.
——, The Red Star Schjellerup 238, 432.
Gravitation, Airy's, 92.
Gravity, Force of, 398.
Green, Defining Powers of Large Telescopes, 124, 239.
——, Markings on Jupiter, 209.
Greenwich Observatory, 278.
Grubb, Adjustment of Equatoreal Telescopes, 9, 43.
Guyot's 'Tables, Meteorological and Physical,' 220.

H*all*, *Asaph*, Commensurability of Motions, 327.
——, Defining Power of Telescopes, 174.
——, Variations of Latitude, 113.
Hall, *Maxwell*, Neptune, 26.
Halley's Southern Catalogue, 429.
Hartnup, J., the late, 443.
Harvard College Observatory, 248, 285.
—— Photometry, 49, 240.
Herschel, Sir W., Relic of, 221.
Holden, Lick Observatory, 84, 432.
Holetschek, Elements and Ephemeris of Comet 1885 *a*, 285.
Hongkong Observatory, 137, 438.
Hough, Large *v.* Small Telescopes, 275.
Huggins, Solar Corona, 153, 376.
Hunt, Hyginus N, 380.
Hyginus N, 294, 380, 433.
Hyperion, Motion of, 92.

Iapetus, Ephemerides of, 33, 64, 96, 318, 348, 400.
Indian Longitudes, 396.
Ingall, Note on Messier, 129.

J*ohnson*, Close Planetary Conjunctions, 306.
——, Old Occultations of Aldebaran, 55.
Jupiter, 76, 209, 211, 268, 273, 300, 374, 378, 383.
——, Ephemerides of Red and White Spots, 31, 63, 95, 141, 190.
Jupiter's Satellites, Abnormal Appearances of, 179, 189.
—— ——, Transits and Nomenclature of, 159.
—— Satellite I. Occulting its Shadow, 159.
—— —— IV., 174, 211.
—— Red Spot, Changes in Length of, 244.

Jupiter's Red Spot, Obscuration of, 172, 214.

Kirch, an Astronomical Family, 305.
Knobel, Alteration of Bye-Law 12, R.A.S., 247.
——, Ptolemy's Catalogue, 87.
Knott, Variable Stars, 128.
——, U Cephei, 178.
Krueger, Ephemeris of Comet 1884 *c* (Wolf), 33.

Lacaille's Observations, New Reduction of, 28.
Lamp, Elements and Ephemeris of Comet Brooks, 348.
Langley's Researches on Solar Heat, 307.
Latitudes, Suspected Variability in, 94, 113.
Lewis, Harvard Photometry, 49.
——, Photometry of Saturn's Rings, 22.
——, The Greenwich System of Sympathetic Clocks and Distribution of Time-signals, 364, 415.
Libration, Lunar Physical, 389.
Lick Observatory, 84, 394, 432, 436.
Light as a means of Investigation, 395.
Liverpool Astronomical Society, Meetings of the, 7, 42, 75, 140, 182, 202, 289, 371.
Longitudes, 129, 396.
Loomis, 'Contributions to Meteorology,' 343.
Lynn, Aldebaran, Occultation of, 86.
——, Bayer's Lettering of the Constellations, 245, 377.
——, Bliss, Astronomer Royal, 86.
——, Brightness of β Aquilæ, 371.
——, Comet 1618, 126, 217.
Lynn, Cornelius Gemma and the Star of 1572, 431.
——, Halley's Southern Catalogue reduced by Sharp, 429.
——, Supposed Comet of 1717 and Comet of 1718, 271.
——, Total Eclipse of May 12, 1876, 270.
Lyræ, β, Variations in the Spectrum of, 129.

Madras Observatory, 220.
Mars in 1884, 282.
——, Diameter of, 135.
——, Oval Spot on, 26.
——, Rotation period of, 437.
Marth, Ephemerides of Iapetus, 33, 64, 96, 318, 348, 400.
Maunder, Motions of Stars in the Line of Sight, 117, 162.
——, New Star in the Great Nebula in Andromeda, 318.
——, Story of the Heavens, 370.
McCormick Observatory, 252.
Mécanique Céleste, 398.
Melbourne Observatory, 136.
Meridian for Australia, 59.
Merope Nebula, 94.
Messier, 94, 129.
Meteor Radiants, 263, 428.
Meteorology, Loomis, 343.
Meteors, 257, 336, 337, 384.
——, Pons-Brooks Comet, 54, 132.
——, and Tempel's Comet of 1866, 386.
——, The Biela Showers, 440.
——, The December Geminids 1884, 216.
Meyer's 'Spaziergänge durch das Reich der Sterne,' 129.
Minor Planets, 30, 62, 63, 221, 285, 347, 399.
Monck, Meteors with Fixed or Stationary Radiants, 428.
——, New Stars and Shooting-Stars, 335.
——, November Meteors, 381.

Monck, Remarkable Meteor-train, 336.
Morrison, Total Solar Eclipse, 1885, March 16, 178.

Natal Observatory, 26, 312.
Nebula, Merope, 94.
——, Proper Motion in a, 127.
Nebulæ, New, 123, 222.
——, Notes on, 210.
Neison, Hyginus N, 433.
Neptune, 26, 111.
——, Satellite of, 318, 346, 349, 400.
New Time, 81, 90, 139, 188, 251, 283, 347, 394, 420.
Nice Observatory, 290, 441.
Notice to Subscribers, 444.
Nutation, Constant of, 313.

Obituary Notices, 222, 444.
Observations, adjustment of, 186.
Observatory, Bordeaux, 439.
——, Brussels, 253.
——, Cambridge, 314.
——, Cincinnati, 311.
——, Dearborn, 135, 441.
——, Dun-Echt, 342.
——, Greenwich, 278.
——, Harvard College, 248, 285.
——, Hongkong, 137, 438.
——, Lick, 84, 394, 432, 436.
——, Madras, 220.
——, McCormick, 252.
——, Melbourne, 136.
——, Natal, 26, 312.
——, Nice, 290, 441.
——, Oxford University, 251.
——, Paris, 280.
——, Stonyhurst College, 312.
——, Strasburg, 344.
——, Temple, 180.
——, Tonquin, 27.
——, Washburn, 60, 344, 432.
——, Washington, 55, 136.
——, Yale College, 91, 312.
Orbits of Meteors, 123.
β Orionis, Spectrum of, 385.
Oxford University Observatory, 251.

Parallax, o^2 Eridani, 345.
—— Hunting, 89.
——, Solar, 134.
——, Solar and Stellar, 212.
——, Σ 2398, 315.
Perrotin, Observations of Double Stars, 392.
Perry, Chromosphere in 1884, 53.
'Phainomena of Aratos,' 397.
Photography, Solar Corona, 218, 277, 376, 388.
——, Stellar, 219, 307.
Photometer, the Wedge, 389, 424.
Photometry of Saturn's Ring, 22.
——, Ceres, Pallas, Vesta, 238.
——, Stellar, 433.
Pickering, Photometry of Neptune, 111.
——, Photometric Observations of Ceres, Pallas, Vesta, 238.
Planetary Conjunctions, close, 306.
Plummer, Harvard Photometry, 240.
Polar Stars, close, reduction of observations of, 187, 313.
Pons-Brooks Comet Meteors, 54.
Prime Meridian Conference, 23.
Prisms, Carbon Bisulphide, 183.
Pritchard, Remarks on Dr. Wilsing's Experimental Examination of the Wedge Photometer, 424.
——, Stellar Photometry, 307.
Pritchett, Jovian Markings, 374.
——, Red Spot on Jupiter, 268.
——, Rotation of Domes, 216.
Proper Motion, Star with large, 188, 399.
—— ——, Lalande, 16616, 293.
Ptolemy's Catalogue, 87.
Pulkowa, The Great Refractor of, 436.
Pyramids and Earth's Axis, 379.

R_ahts_, Elements and Ephemeris of Tuttle's Comet, 284, 347.
Red Star Schjellerup 238, 432.
Red Stars, New, 30.
—— ——, Observations of, 400.
—— ——, Two small, 124.
Resal's 'Mécanique Céleste,' 398.
ROYAL ASTRONOMICAL SOCIETY, MEETINGS OF, 1, 37, 69, 101, 145, 193, 196, 225, 405.
—— —— ——, Proposed Alterations in the Bye-Laws of, 179.
—— —— ——, Alteration of Bye-Law 12, 247.

S_adler_, Note on a Possible Case of Proper Motion in a Nebula, 127.
Saturn, Conjunctions of, with Small Stars, 391.
Saturn's Rings, Photometry of, 22.
—— ——, Variability of, 181.
—— Satellites, Times of East Elongations of, 34, 66, 97, 317, 350, 401.
—— ——, Inner, 93.
Sawyer, Pons-Brooks Comet Meteors, 54.
——, December Geminids, 1884, 216.
Saxby, Spectroscope in the Alps, 357.
——, Kayser on the Spectroscope, 427.
Schellen's 'Spectrum Analysis,' 295.
Shearmen, Photographing the Solar Corona by a New Method, 277.
Society for Popular Astronomy at Basle, 61.
Solar Heat, Langley's Researches on, 307.
Souchon's 'Traité d'Astronomie Pratique,' 329.
Southern Circumpolar Stars, Standard, 250.
'Spaziergänge durch das Reich der Sterne,' 129.
Spectra, Third-Type, 383.
Spectroscope in the Alps, 357.
——, Kayser on, 427.
Spectroscopic Determination of the Motions of Stars, 117, 162.
Spectrum, Influence of Temperature on the Character of the Lines of, 132.
—— of Great Nebula in Andromeda, 385.
—— of β Lyræ, Variations in, 139.
—— of β Orionis, 385.
Spitta, Bright Meteor, 337.
——, Markings on Jupiter, 273.
——, Satellite IV. of Jupiter, 174.
——, Satellites of Jupiter, Abnormal Appearances of, 179.
Standards of Stellar Magnitudes, Second Report of the Committee on, 51.
——, Third Report of the Committee on, 436.
——, Faint Stars, 62.
Stars, New, and Shooting-Stars, 335.
Stellar Distribution, 134.
—— Magnitudes, 51, 436.
—— Photography at Paris, 219.
Stokes's 'Light as a Means of Investigation,' 395.
Stonyhurst College Observatory, 312.
Story of the Heavens, 370.
Stothert, Aldebaran, Occultation of, 128.
——, Nice Observatory, 290.
Strasburg Observatory, 344.
'Sun and his Phenomena, The,' 181.

Tables, Meteorological and Physical, 220.
Tebbutt, Recent Measures of α Centauri, 382.
Telescopes, Defining Powers of Large and Small, 76, 124, 174, 205, 238, 275, 300, 340, 342, 391.

Tempel Comet II., 1866, and the November Meteors, 386.
—— —— 1867, Elements and Ephemeris of, 140.
—— ——, 210.
—— ——, Alleged Discovery of, 221.
Temple Observatory, 180.
Time-signals, Distribution of, 364, 415.
Tonquin Observatory, 27.
'Traité d'Astronomie Pratique,' 329.
Transit Formulæ, Approximate, 130.
Trouvelot, Oval Spot on Mars, 26.
Tupman, Biela's Comet and Comet *f* 1881 (Denning), 257, 306.
Tuttle's Comet, 284, 311, 347.

Universal Time, 81, 90, 139, 188, 251, 347, 394.
Uranus, Mass of, 312.
——, Outer Satellites of, 345.

Variability in Terrestrial Latitudes, Suspected, 94, 113.
—— of Saturn's Rings, 181.
Variable Star, U Cygni, 20.
—— ——, U Cephei, 178.
—— ——, New, 62, 180, 222, 252.
—— Stars, 32, 65, 98, 128, 142, 189, 222, 253, 286, 315, 349, 402.
Variable Stars, Maxima and Minima of, in 1884, 170, 172.
—— ——, Observations of, in 1884, 251, 399.
—— ——, Suspected, 249.
Variations in the Spectrum of β Lyræ, 139.
Venus, Transit of, Solar Parallax from Photographs of, 134.

W*alker*, Pyramids and the Earth's Axis, 379.
Washburn Observatory, Publications of, 60, 344, 432.
Washington Observatory, 55, 136.
Webb, Rev. T. W., the late, 222.
——, The Sun and his Phenomena, 181.
Wedge Photometer, 389, 424, 442.
Wendell, Comet-Meteor Radiants, 263.
Wolf's Comet 1884 *c*, Ephemeris of, 33.
—— ——, Transit of, over two Stars, 94.

Yale College Observatory, 91, 312.
Yarnall's Catalogue, Anonymous Stars of, 387.
Young, Obscuration of the Red Spot on Jupiter, 172.
——, On the Solar Corona, 183.

SUPPLEMENT TO No. 93.

Mean Places of Variable Stars for 1885·0.

PLATE.

Page
Apparent Orbits of the Seven Inner Satellites of Saturn . . *to face* 34

ENGRAVINGS.

Adjustment of Equatoreal Telescopes 12, 13, 14, 48
F line in Spectrum of Sirius compared with Hβ line of Hydrogen, 1875 December and in 1884 . . . . 120, 168
Encke's Comet 122
Equatoreal White Spot and Shadows of Satellites II. and III. (Jupiter) 160
Region near F in Spectra of the First and Second Types 163
Chart showing distribution of Stars observed for motion in the line of sight 166
Chart of separate observations of motion of Sirius in the line of sight 168
On Denning's and Biela's Comets 262
Jupiter 269
Section of Dome of the Nice Observatory 292
Hyginus N 295
Apparent Orbits of the Seven Inner Satellites of Saturn 316
Comet 1812 (Pons-Brooks) 326
Barometric and Temperature Adjustments of the Greenwich Sidereal Standard Clock 365
Greenwich Sidereal and Mean Solar Clock Circuits 367
Circuit for the Time-signals to General Post Office 416
Greenwich Time-ball 417
Portion of Post-Office Chronopher 419

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 93. JANUARY. 1885.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

December 12, 1884.

E. Dunkin, F.R.S., *President*, in the Chair.

Secretaries: Lieut.-Col. G. L. Tupman, and
A. M. W. Downing, M.A. (*acting for Mr. Knobel*).

The Minutes of the last Meeting were read and confirmed.

Col. Tupman announced that 52 presents had been received since the last Meeting, the only one calling for special notice being the second volume of the republication of Dembowski's Double Star Observations.

The thanks of the Society were unanimously voted to the respective donors.

The President. It is necessary at this meeting to appoint Auditors to audit the Treasurer's account. I shall be glad if some gentleman, not a Member of the Council, will propose three names.

Mr. Talmage. I beg to propose the names of Mr. Lecky, Mr. Inwards, and Mr. Maunder, as Auditors.

Sir J. Cockle. I have much pleasure in seconding that motion.

The motion was put and carried.

Mr. Spitta read a paper on "A new occulting eyepiece," which he had proved to be successful, convenient, and economical. It consisted of a small brass cylindrical box, with four shutters placed in front of the tube carrying the lens. These shutters were arranged in such a manner that the observer could bring them all into focus at the same time, and by a little manipulation the field of view might be reduced to a slit or even a pin-hole. The uses of the instrument were various, viz.:—to occult the bright portion of an object whilst examining its fainter parts; for finding faint companions of bright stars; for examining the satellites of Jupiter during transits; for observing fainter portions of clusters, and so on. Probably Mr. Common and other photographers of celestial objects might find it useful in blotting out a portion of the field when too bright, and taking a second negative, and afterwards adopting double printing. It could also be used to determine approximate position-angles.

Mr. Spitta was aware that it might be argued that a bar and an ordinary eyepiece would answer the same purpose; but that required a good clock to keep the bar in position; and then the glare is on both sides; but in this instrument it *could* only be on one side. There were some who used a Dawes eyepiece as an occulting agent, but that was apt to occult the very part it was desired to observe.

The President. I am sure we must all thank Mr. Spitta for his paper, and the beautiful eyepiece he has produced, which I have no doubt will also prove a very useful one.

Mr. Downing then read the general conclusions at which Dr. Gill had arrived in a paper "On Systematic Errors in the readings of the Circle Microscopes of the Cape Transit Circle":—

1. That the errors of the screws of transit circles require much more frequent examination than it has been customary to give them.

2. That steel screws should be substituted for the gun-metal screws in the Cape Circle microscopes.

3. That the screws in three of the microscope-micrometers should be reversed, as at Greenwich, so that the springs may press the bearings of the screws in opposite directions.

4. The N. P. D. results of the Cape Catalogue for 1880 must be corrected for screw errors, and the determination of latitude be rediscussed, before they can be admitted into fundamental determinations.

The President. I may say that the wearing of screws has caused a great deal of trouble at the Greenwich Observatory. Mr. Christie has published a paper showing the effects of the systematic errors of the screws, the consequence of which has been that the screws of three micrometers have been reversed, so that the effects of the wearing of the screws in future will be eliminated. Of course in the case of meridian observations it is highly important that all instrumental errors should be found out. We look upon Mr. Stone's large catalogue of 12,400 stars as a great work, and high praise is due to him for the energy he showed during the ten years he was at the Cape, and our knowledge of the places of the stars in the southern hemisphere will, for some considerable time to come, owe very much to the labours of Mr. Stone. It appears that Mr. Gill, since he has been at the Cape, has discovered that the micrometer-screws of the Cape transit circle—which is an exact copy of the Greenwich transit circle and was made by the same makers, so that the screws would be likely to wear in the same degree—are wearing away, and to that cause is due certain systematic errors. Mr. Stone knows that errors of this kind will take place, and that it is impossible to produce such an extensive catalogue as he has done without its containing some systematic errors.

Mr. Stone felt that Mr. Gill had overstated the case in regard to these systematic errors. The tests he applied—comparing the nadir point over a considerable range of the screws, determining the flexure, and also the runs of the micrometer-screws, and shifting the reading of the circle mircoscopes relative to the comb—substantially freed the work from systematic errors arising from the wear of different parts of the screws used. In many of these discussions Mr. Gill seemed to have entirely overlooked the fact that in adjusting the instrument the comb is being continually shifted relatively to the screw, and hence that part of the reading supposed to be 0 rev. at one time, does not necessarily correspond with 0 rev. at another time. Unless you know what changes occur in adjusting the comb, you cannot argue from a mere comparison what the corrections of the different parts of the screw are. He could not say positively that the constraint referred to by Mr. Gill occurred always at the 0 rev. in the observations of 1856 and 1857, as they did in the time between 1877 and 1878. Mr. Gill assumed that all these things had been left practically unchanged. He contended that all practicable tests had been applied. He did not profess to have made better observations with the instrument than others could have made with it; but he certainly did claim to have made as good as the instruments at his command could turn out. He would be very glad indeed to see Mr. Gill's observations if he could do anything greatly better.

The thanks of the meeting were voted to Mr. Gill for his paper.

Mr. Downing read a short paper on the right ascensions of certain standard polar stars by Prof. Safford, who advocated the more systematic observation of stars of high northern declination. Some researches had been made, but they were, for the most part, unpublished. It would be desirable to insert stars of a somewhat fainter magnitude than those which are under observation in other parts of the heavens in the working star catalogues of different observatories, for stars of the class under consideration.

The thanks of the Society were accorded for the paper.

Col. Tupman exhibited an elaborate chart by Mr. Marth, and explained that it was a representation of the orbits of the planets, including many of the minor planets, and some comets, on a plane perpendicular to the ecliptic, which was supposed to rotate with the Sun as a centre, so that every orbit would describe a curve upon it as it rotated. Mr. Marth had not sent a description.

Mr. Common. As far as I understand it, the idea is this: that if this is a plane through the centre of the Sun and perpendicular to the ecliptic, you would have an exactly circular orbit described by a dot; if the orbit was in the plane of the ecliptic and eccentric but not inclined, it would be represented by a straight line; if the orbit were not in the plane of the ecliptic it would be shown by an irregular curve.

Further discussion was adjourned till Mr. Marth had forwarded a description.

Col. Tupman. We have a long paper from Mr. Denning on the long duration of meteoric radiant-points, an exceedingly interesting paper to those specially interested in the subject, and which contains the results of a vast number of observations made during a great many years. He demonstrated the duration of these radiants by tracing them independently from week to week, so that he gradually grew to the conviction that these radiant-points were really persistent at definite points for months together. He communicated his ideas to Professor Herschel and others, but they had a difficulty in reconciling such phenomena with theory. However, Mr. Denning set to work, notwithstanding this rebuff, and has made these observations carefully and well, and has now demonstrated, from his own observations and from those of others also, that a great number of radiants last for several months. There is no sort of doubt of the existence of these radiants. What the explanation is I do not know. [*Colonel Tupman* then made a rough copy of one of Mr. Denning's charts on the blackboard.]

Mr. Ranyard. There is another point which Colonel Tupman has not referred to. These little groups which Colonel Tupman has been drawing on the board are in every case, Mr. Denning says, meteors of the same type. Meteoric observers know that meteors from the same radiant have a family likeness—that those from some radiants are swift, from others coloured, from others streak-leaving, these peculiarities constituting a sort of family likeness. The meteors coming from the little groups Colonel Tupman has drawn are of similar character for each group, which is a strong reason for supposing that they come really from the same radiant-point, and not merely from another radiant-point in the neighbourhood a degree or two apart. I have asked Mr. Denning with what accuracy he can determine the radiants. When there is a fair number of good observations he thinks he can then determine the position of the radiant within a degree. He has found such radiants lasting from one six months to another when the earth is moving in opposite directions, which means that the meteors must be moving enormously swiftly as compared with ourselves, if we are meeting them in space. If the radiants do not move a degree in the interval they must be moving 53 times as fast as we are, or with 20 times the velocity of the great November showers, which is 44 miles a second. The duration of the November meteors is, say, a second and a half or two seconds, the length of time differing but little whether we see them streaming across the horizon or coming straight at us. Usually we know how very short is their duration. Well if these meteors are travelling 20 times as fast, the duration would be a very small fraction of a second, if they move with this velocity the whole time that they are burning out. It has occurred to me that the explanation may be that these small meteors may lose a great portion of their velocity in our upper air, before they attract our attention by becoming incandescent. We know little about the rate at which a meteor loses its velocity. Most of them become visible at a height of 100 miles, and are burnt out before they get to a depth of 60 miles in our atmosphere. If these meteors from the fixed radiants attract our attention only when they have lost the greater part of their velocity, they may have had an original velocity 20 or 25 times as great as the November shower. But otherwise the question is of the greatest difficulty, and it is impossible to reconcile with ordinary theory, that the meteor-radiants should not shift. I may add that Mr. Denning has observed the shifting of some of the planetary meteoric showers, and finds that they shift backward as they ought, supposing the meteors all to be moving round the Sun in different planes, and taking into account the different directions of the Earth's motion at the different parts of the year. He has sent me a list of radiants near Capella extending over nine months, and all within a couple of degrees. They are separate groups of determinations, sometimes made by different observers. He takes also the observations of other meteoric observers, and he finds they agree with his own. There is no group that he has found going on the whole year round, because the meteors cannot be observed when in the same right ascension as the Sun; otherwise his observations point to the fact that the fixed radiants are probably active all the year round: that is to say, we are moving through a hail of little particles moving in different directions in space. Mr. Denning has a theory that possibly these fixed radiants may be due to streams of meteors moving round the Earth, but I see great difficulties in accepting this view.

Mr. Waters. Is there no sort of explanation of this? Is it not possible that these meteors accompany the Earth, and at some point of their orbit they enter the atmosphere at the nearest point?

Colonel Tupman. That is not likely, because any such system would be very quickly exhausted, in a single revolution, while passing through perigee.

Mr. Waters. How long a period have these observations extended over?

Colonel Tupman. I think about 15 years or more.

Mr. Waters. Observations of the same point year by year?

Colonel Tupman. I believe it is Mr. Denning's opinion that if you were to put all his observations in the fire, and begin again and go on for another 15 years, you would be precisely in the same place as now—that these radiant-points would be clustered very nearly together, only differing in position, or by the probable error of the determination.

Mr. Rand Capron suggested the great importance of the spectroscopic examination of meteors, and pointed out that it had been done to some extent by Dr. Konkoly in Hungary, Mr. Browning, and some other observers.

Mr. Maunder. Has Mr. Denning found any radiants behind the

Earth, *i. e.* in the part of the heavens from which the Earth is proceeding in its orbit?

Colonel Tupman. Yes, I think there are a very few. You cannot expect to have many, but there are some.

Mr. Maunder. Has any difference in the character of the meteors been noticed when the Earth is proceeding towards the radiant and when it is receding from it, because, of course, the meteors will meet the Earth with a difference of 36 miles a second in their speed?

Colonel Tupman. It will make a difference in the velocity clearly. The meteors that overtake the Earth are reddish in colour. Nearly all the radiants that come from what is called the antiapex of the Earth's way are of a reddish tint, as though they were of very slow velocity.

Mr. Ranyard. If the meteors are moving 50 times as fast as the Earth, there would be little difference in their numbers whether they met the Earth or overtook it. All observers, however, know that meteors are much more abundant in the morning than in the evening; therefore a large number of meteors cannot belong to meteors coming up out of space with this enormous velocity. That shows they must be moving with a velocity more comparable with our own velocity.

The President. I am sure we must thank Mr. Denning, and not only Mr. Denning, but also Colonel Tupman, for this interesting account.

Mr. Downing read a paper on spectroscopic observations of nebulæ made at Dun Echt by Dr. Copeland. It was mentioned in the paper that the comet Wolf was found 12 hours before the telegraphic announcement, which was delayed in transmission.

The thanks of the Society were voted for the paper.

Mr. Chambers. May I ask what has become of the 'Monthly Notices' for November?

The President. I believe it is out. It was only received from the printer last night, and was posted early this morning. It is a very thick number, and has given a great deal of trouble in passing it through the press.

Mr. Chambers. I am not complaining of the printer or the officials in the matter.

The following papers were also announced:—

Communicated by the *Astronomer Royal.* "Observations of Comet *c* 1884 (Wolf), made at the Royal Observatory, Greenwich."

C. Gogou. "On the Numerical Value of the Coefficient of Neison's long Inequality in the Moon's Motion, due to the action of Mars [extract from a letter to Prof. Adams]."

A. B. Biggs. "Observations of Comets Pons-Brooks and Ross, 1884."

The following gentlemen were elected Fellows of the Society:— H. Park Hollis, B.A.; Thos. Hovendon; Thos. Lewis; Lord Mac-

Laren; Rev. W. J. Baden Roome; Harold Seward; Rev. Henry Wheaton; and Rev. J. Wilkins.

The Meeting adjourned at 9^h 30^m P.M.

Meeting of the Liverpool Astronomical Society.

THE usual monthly Meeting was held on Dec. 8 at the Association Hall, the Rev. T. E. Espin, B.A., President, in the chair. A paper, by Prof. E. C. Pickering, of Harvard, was read, on "The Reduction of Observations for Atmospheric Absorption at Low Altitudes." The observations in question were made by Mr. Espin in July last on the Rigi, the object being to test the amount of light that was absorbed by the air when stars were low down on the horizon. Upwards of 50 stars, with a zenith-distance of from 80° to 88°, were observed and compared with others of the same magnitude overhead, and, in some of the lowest, the loss of light was 70 per cent.; so that it was very doubtful whether stars below the third magnitude would be visible to the naked eye at less than 2° above the horizon. Further observations of stars near the horizon were greatly needed; and it was to be hoped that other members of the Society would take up the question. In speaking of the results of the International Meridian Conference, the President said the Astronomer Royal proposed, after the 1st of January, to adopt the new method of reckoning time at Greenwich. He thought the change would be of great advantage to astronomers, and he would move that the Liverpool Society follow the same course.

Mr. T. W. Backhouse, F.R.A.S. (Sunderland), thought the only objection could be the existence of the 'Nautical Almanac' for several years yet to come with ephemerides calculated for hours reckoned from noon. This was an inconvenience they must consider; but it seemed to him much better to adopt the recommendation of the Conference than wait an indefinite time for the change. Some authoritative decision should at once be announced, and very fittingly by the Royal Astronomical and Liverpool Societies.

Mr. W. S. Franks, F.R.A.S. (Leicester), said that Englishmen ought to be proud that Greenwich was selected to be the timekeeper for the world. He thoroughly agreed with Mr. Backhouse that an authoritative decision should be arrived at, else what was the use of appointing an international conference of such distinguished men? Twenty-four-hour dials had already been introduced to the trade, and the manufacturers would scarcely have ventured on this if they had not felt sure it would be ultimately adopted. The striking of the hours was certainly a knotty point, and it would be rather too bad if, just as one was comfortably settled for the night, all the clocks were to begin striking 24. (Laughter.)

Mr. R. C. Johnson, F.R.A.S., thought that, so far as the 'Nautical Almanac' was concerned, it would be a very easy matter to subtract 12 hours from any given time, whereas at New York they would have to begin their astronomical day at 5 hours, and allow one hour for every 15° west. France had not yet given her adhesion to the plan; but, as there was very little difference between the meridians of Greenwich and Paris, it was very likely they would move their own meridian westward, and so compromise the matter without hurting their pride. After some further discussion the question was put to the meeting and carried unanimously.

Mr. J. Baxendell, jun., communicated a paper on the results of a discussion of 34 maxima and 18 minima of the variable star S Ursæ Majoris, which he had collected from various sources. These results gave a decided lengthening of the period. From 1855 to 1865 the mean period was 224·071 days, from 1865 to 1875 it was 220·425 days, and from 1875 to 1884 231·236 days. From 15 of the minima corresponding to the period 1865–75, he obtained a mean period of 225·334 days, which it would be seen was only 0·091 of a day less than that derived from the corresponding group of maxima.

In some notes contributed by Mr. J. E. Gore, F.R.A.S., on some suspected variable stars, he included B.A.C. 740 Cassiopeiæ, Bradley 396 Cassiopeiæ, 39 Aurigæ, and Birmingham's 569 Delphini. The latter had been suspected of variation by d'Arrest, who rated it 8^m on Sept. 10, 1874. His own observations since Sept. last would show that this star was certainly variable and deserving of careful observation.

In a paper on Red Stars Mr. S. M. B. Gemmill said he had found this class of observation much easier than it was generally supposed to be. The diversity of opinion which seemed to exist as to the tints of stars, and especially of red stars, was very striking, and sufficient allowance did not appear to be made for personal and instrumental equation. He thought the designation of *red* was, for the most part, a misnomer, as it was to be questioned whether there was any really red star in the sky. With the exception of certain ruby or crimson stars, he had never seen one which had not a mixture of orange or yellow, and, even in the former class, he thought the tints were not pure.

On the subject of the projection of stars during lunar occultation, the Rev. S. J. Johnson, M.A., F.R.A.S., said the approaching occultation of Aldebaran, visible in this country, led one to look up past cases of the illusion of projection of stars on the Moon's limb. The occultation of α^2 Libræ, communicated to the Society last session, was an instance that this phenomenon was not confined to red stars. In the 'R.A.S. Monthly Notices' for June 1828 a list of stars was given that furnished instances of peculiarities in occultations, and it might be as well to note the five different explanations there given:—

1. A lively imagination on the part of the observer.

*Mean Places of Variable Stars for 1885·0.**

Star.	R.A.	Dec.	Star.	R.A.	Dec.
	h m s	° ′		h m s	° ′
T Cassiop.	0 17 1	+55 9·3	δ Libræ	14 54 50	− 8 3·7
R Androm.	0 17 58	+37 56·4	T Libræ	15 4 11	−19 34·8
S Ceti	0 18 12	− 9 57·9	U Coronæ	15 13 30	+32 4·1
U Cephei	0 52 8	+81 15·3	S Libræ	15 14 48	−19 58·3
S Cassiop.	1 11 13	+72 0·3	S Serpent.	15 16 17	+14 43·6
S Piscium	1 11 34	+ 8 19·8	S Coronæ	15 16 43	+31 46·9
R Piscium	1 24 42	+ 2 17·3	R Serpent.	15 45 24	+15 29·0
S Arietis	1 58 28	+11 58·5	V Coronæ	15 45 25	+39 55·1
R Arietis	2 9 35	+24 31·2	R Libræ	15 47 5	−15 53·6
o Ceti	2 13 32	− 3 30·0	R Hercul.	16 1 3	+18 40·9
R Ceti	2 20 9	− 0 41·9	W Scorpii	16 5 3	−19 50·2
T Arietis	2 41 55	+17 1·8	R Scorpii	16 10 48	−22 39·6
β Pers. Algol.	3 0 41	+40 30·7	S Scorpii	16 10 49	−22 36·5
R Persei	3 22 44	+35 16·7	W Ophiuch.	16 15 13	− 7 25·5
λ Tauri	3 54 19	+12 9·9	V Ophiuch.	16 20 21	−12 9·4
R Tauri	4 22 0	+ 9 54·3	U Hercul.	16 20 44	+19 9·1
S Tauri	4 22 54	+ 9 41·4	T Ophiuch.	16 27 9	−15 53·1
V Tauri	4 45 22	+17 20·6	S Ophiuch.	16 27 38	−16 55·0
R Orionis	4 52 46	+ 7 57·3	W Hercul.	16 31 8	+37 34·5
R Leporis	4 54 22	−14 58·8	R Dracon.	16 32 21	+66 59·9
R Aurigæ	5 8 1	+53 27·3	S Hercul.	16 46 39	+15 8·2
S Orionis	5 23 20	− 4 47·1	R Ophiuch.	17 1 10	−15 56·3
η Gemin.	6 7 56	+22 32·4	U Ophiuch.	17 10 41	+ 1 21·4
T Monocer.	6 19 1	+ 7 8·9	X Sagittar.	17 40 19	−27 47·1
R Lyncis	6 51 49	+55 29·4	W Sagittar.	17 57 41	−29 35·1
ζ Gemin.	6 57 17	+20 44·3	T Hercul.	18 4 45	+31 0·1
R Gemin.	7 0 26	+22 52·8	T Serpent.	18 23 12	+ 6 13·4
R Can. Min.	7 2 23	+10 12·2	U Sagittar.	18 25 7	−19 12·4
V Gemin.	7 16 42	+13 18·7	R Scuti	18 41 21	− 5 49·7
U Monocer.	7 25 19	− 9 32·2	β Lyræ	18 45 50	+33 13·8
S Can. Min.	7 26 29	+ 8 33·8	R Lyræ	18 51 50	+43 47·7
T Can. Min.	7 27 36	+11 59·4	S Cor. Aust.	18 53 23	−37 6·9
U Can. Min.	7 35 5	+ 8 38·9	R Aquilæ	19 0 50	+ 8 3·4
S Gemin.	7 36 9	+23 43·2	T Sagittar.	19 9 36	−17 10·3
T Gemin.	7 42 24	+24 1·2	R Sagittar.	19 9 57	−19 30·6
U Puppis	7 55 28	−12 32·8	S Sagittar.	19 12 42	−19 13·9
R Cancri	8 10 13	+12 4·7	R Cygni	19 33 44	+49 56·6
V Cancri	8 15 10	+17 39·0	S Vulpec.	19 43 41	+27 0·1
U Cancri	8 29 11	+19 17·5	χ Cygni	19 46 9	+32 37·4
S Cancri	8 37 22	+19 26·8	η Aquilæ	19 46 37	+ 0 42·7
S Hydræ	8 47 34	+ 3 30·1	S Cygni	20 3 5	+57 39·3
T Hydræ	8 50 4	− 8 42·2	R Capric.	20 4 51	−14 36·5
T Cancri	8 50 6	+20 17·3	S Aquilæ	20 6 20	+15 16·8
R Leo. Min.	9 38 41	+35 2·5	R Sagittæ	20 8 50	+16 22·7
R Leonis	9 41 23	+11 57·7	R Delphin.	20 9 22	+ 8 44·5
R Urs. Maj.	10 36 30	+69 22·7	U Cygni	20 16 2	+47 31·9
R Crater.	10 54 54	−17 42·4	V Cygni.	20 37 36	+47 43·8
S Leonis	11 4 54	+ 6 5·1	S Delphin.	20 37 47	+16 40·5
R Comæ	11 58 21	+19 25·5	T Delphin.	20 40 2	+15 58·9
T Virgin.	12 8 43	− 5 23·8	U Capric.	20 41 44	−15 12·3
R Corvi	12 13 40	−18 36·9	T Aquar.	20 43 52	− 5 34·4
T Urs. Maj.	12 31 9	+60 7·2	R Vulpec.	20 59 17	+23 22·0
R Virgin.	12 32 40	+ 7 37·3	V Capric.	21 1 58	−21 48·6
S Urs. Maj.	12 38 55	+61 43·4	T Cephei	21 8 1	+68 1·3
U Virgin.	12 45 16	+ 6 10·8	T Capric.	21 15 40	−15 38·7
W Virgin.	13 20 6	− 2 46·9	S Cephei	21 36 38	+78 6·4
V Virgin.	13 21 52	− 2 34·6	T Pegasi	22 3 17	+11 58·7
R Hydræ	13 23 26	−22 41·2	δ Cephei	22 24 54	+57 49·6
S Virgin.	13 27 0	− 6 36·2	R Lacertæ.	22 38 10	+41 46·1
Z Virgin.	14 4 9	−12 45·5	S Aquar.	22 50 57	−20 57·4
S Boötis	14 19 2	+54 20·1	R Pegasi	23 0 52	+ 9 55·4
R Camel.	14 26 20	+84 21·2	S Pegasi	23 14 44	+ 8 17·4
R Boötis	14 32 7	+27 14·2	R Aquar.	23 37 52	−15 55·3
U Boötis	14 49 0	+18 9·8	R Cassiop.	23 52 34	+50 44·9

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* *From the* ‘*Annuaire* du Bureau des Longitudes,’ 1885.

2. A spurious disk given to the Moon's image by the instrument of observation.
3. A lunar atmosphere.
4. Irradiation.
5. Different refrangibilities to which the rays from the Moon and star are liable arising from their differences of colour.

He (Mr. Johnson) had noticed the lingering of stars on the lunar disk in the greatest degree during total eclipses, especially in that of Aug. 23, 1877, when even minute stars appeared several seconds within the reddening limb; though on the occasion of the last eclipse the Moon presented such a remarkable obscurity of tint that only one instance of this kind appeared, and that not very conspicuously.

On the subject of the November Leonids 1884, Mr. E. F. Sawyer, of Cambridgeport, U.S.A., said this shower appeared to be very inactive, as during watches of $1^h\ 40^m$ on the mornings of Nov. 13, 14, and 15, only two faint meteors were observed which could be attributed to the Leonid radiant. During these periods ten meteors were recorded which were all quite faint, not exceeding a third-magnitude star in brightness; whilst their paths were invariably short.

The President said there had been some talk about forming an educational branch for Liverpool. Father Perry had aroused an enthusiasm for Astronomy, and he thought it would be a pity if the result of his good work was suffered to die out. It had therefore been proposed to hold elementary classes fortnightly during the winter, and to deliver illustrated lectures on popular subjects.

Mr. Joseph Baxendell, F.R.S., thought the number of people who took any interest in Astronomy was extremely small and proportionally less than it was twenty-five years ago.

Mr. W. H. Davies (Secretary) said they had certainly found it very up-hill work in Liverpool; but, with beginners, a great deal depended upon the way the subject was treated.

Mr. Isaac Roberts, F.G.S., F.R.A.S., suggested that the classes might combine Astronomy and Navigation and be in connection with the South Kensington scheme.

Eventually it was decided to hold classes for the study of Astronomy only, and Mr. J. P. Weir, 15 Selborne Street, was elected Secretary of the Branch.

Owing to the pressure of business, other papers were taken as read.

Eight gentlemen were elected Members of the Society, and twenty-six candidates were proposed.

On the Adjustment of Equatoreal Telescopes [*].

In submitting the following notes I wish it to be distinctly understood that I do not claim any novelty whatsoever for any of the methods proposed. All books on Astronomy give directions for adjusting telescopes, differing very widely one from another, and the amateur is sometimes puzzled as to the best method to adopt. I give in the adjoined notes the result of my own experience and the methods I myself have found best under various circumstances and conditions, and I shall be pleased if these notes prove at any time useful to Members of the Society.

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[*] Read before the Liverpool Astronomical Society at the Meeting held on Monday, Oct. 13, 1884.

An Equatoreal Telescope that is in proper adjustment has its polar axis parallel to the axis of the earth, its declination axis at right angles to its polar axis, and the optical axis of the telescope at right angles to the declination axis. Also its circles are so placed that when the optical axis of the telescope is directed to the imaginary point of the heavens found by the intersection of the Equator and the Meridian, the declination-circle will read exactly 0° if numbered for declination readings, or 90° if numbered for Polar-distance readings, plus or minus a small quantity due to refraction, and the R.A. circle will read XXIV or XII according to whether the telescope is placed at E. or W. of pier.

In order that these conditions be fulfilled, certain adjustments are necessary after the instrument is roughly placed into position. The adjustments of the Equatoreal are generally stated as follows:—

1. Adjustment of polar axis to the altitude of the Pole.
2. Adjustment of the zero-point of declination-circle.
3. Adjustment of polar axis to Meridian.
4. Adjustment of optical axis at right angles to declination axis.
5. Adjustment of polar and declination axes at right angles to each other.
6. Adjustment of the zero-point of hour-circle.

To these may be added, adjustment of optical axis of finder parallel to optical axis of telescope.

In making these adjustments it will be found necessary, as stated in all works on the subject, to repeat them several times, because the errors to some extent affect each other. It is desirable, therefore, to take the adjustments in such order as will render each, as far as possible, independent of those that follow.

In practice, therefore, I find this order best:—

1. Adjust finder parallel to telescope.
2. Adjust telescope at right angles to declination axis.
3. Adjust declination axis at right angles to polar axis.
4. Adjust zero of declination-circle.
5. Adjust inclination of polar axis.
6. Adjust polar axis into Meridian.
7. Set zero of R.A. circle.

It will be seen as we go on that by a little care in the selection of objects, &c., each of these adjustments can be made almost independent of the errors of those following.

1. *To adjust Optical Axis of Finder into Parallelism with the Optical Axis of Telescope.*—As all celestial objects are at great distances this is the same as saying that both telescope and finder should point exactly to same object at same instant. To test this

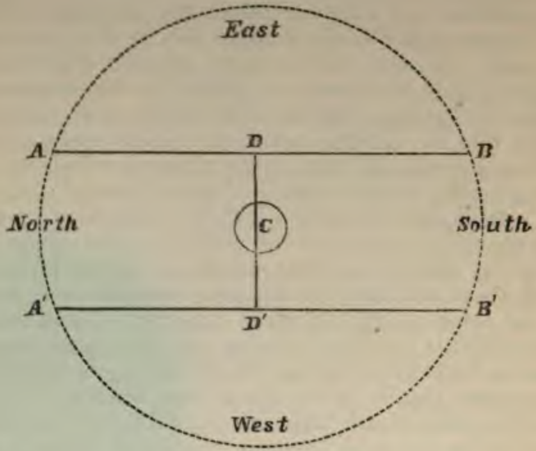
crucially it is necessary to define exactly the centre of the field of telescope. A pair of cross-lines in an eyepiece, or what is generally called a "Transit" eyepiece, is generally sufficient for this; but if a micrometer is to be used, and great exactitude is required, the micrometer may be placed in position and the centre found by rotating it (the micrometer) on its position-circle and altering the position of the wires by the screw-head till the intersection of the position and distance wires (vertical and horizontal) appears to remain stationary on whatever object the telescope be pointed; probably a terrestrial object is best for this, as clockwork need not then be used. If now a star or other celestial object be brought into centre of field of telescope, as defined by intersection of wires of micrometer, the same star should appear on cross-lines of finder on looking into it. If it is not so the finder needs adjustment. In small equatoreals, and particularly when the tube is parallel, the finder is generally rigidly attached to main telescope; but the wire plate, *i. e.* the metallic ring to which the cross wires are attached, has four adjusting screws, two of which, if rightly placed, should be parallel to, and two at right angles to the line of declination axis, and these will allow of the ring carrying the whole set of cross-lines being adjusted a small quantity. These screws are generally made as "pulling" screws, *i. e.* they pass through the tube and are tapped into the wire plate. Whatever side, therefore, the cross-wires require to be moved *from*, the screw at that side should be loosened and the opposite one tightened; care should be taken not to use too much force, for the screws act so powerfully on the ring that it is not an unfrequent occurrence to find the wires broken, the ring being forced out of shape by too much force on the pulling screws. If the adjustment required be too much for the wire plate, or that it is necessary to draw the cross-wires so much to one side as is decidedly apparent on looking through the eyepiece, it is better to make a rough adjustment by the finder supports; a slight slotting of the holes by which these are attached to tube or a piece of cardboard under one or other generally brings the adjustment very nearly right, and avoids the necessity of placing the cross-wires sensibly eccentric in the field. In large instruments there is generally an adjustment in the supports themselves; sometimes both vertical and horizontal adjustments are on one support, but more generally the vertical adjustment is on one and the horizontal on the other; this is the plan usually adopted when the telescope-tube is large and taper, for it is the more difficult to make the adjustment mechanically in the workshop in that case. The first adjustment is perfectly independent of any other, and I prefer to make it first for this reason, and also because it is convenient in making other adjustments to have a well-adjusted finder to the telescope.

2. *To adjust Optical Axis of Telescope at Right Angles to Declination Axis.*—As I consider the reader will be much better able to make these several adjustments satisfactorily if he has an

intelligent appreciation of the *principles* on which he proceeds, I purpose explaining each as I go on.

For sake of simplicity, suppose it were possible to find a star exactly on the equator and in the meridian, and, moreover, that we could induce that star to remain apparently stationary during the time of our observation.

Fig. 1.



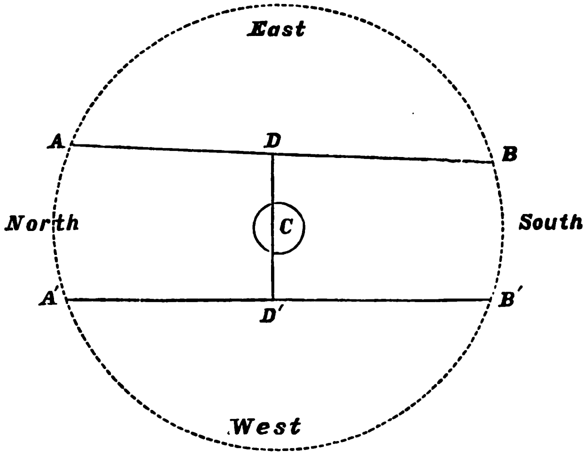
If now in figure 1 A B represents the axis of the telescope, and C D the declination axis, all looked at from direction of Pole. Suppose the telescope be pointed at this ideal star, it (the telescope) being at E. side of stand, and then that it be turned to other side of pier (W.), and again pointed to it as at A′ B′. If the telescope be exactly at right angles to declination axis, *i. e.* if A D C and B D C be 90° then A′ D′ C and B′ D′ C will also be right angles, and the second position of telescope will be exactly parallel to the first. The instrument must, therefore, have been turned through exactly 180° on its polar axis; but if the telescope be not quite at right angles to declination axis, if, suppose the angle at A D C be greater than B D C, then it is evident that the whole instrument must be turned through more than 180° before the second position of telescope be made parallel to the first, for if moved 180° only, the two positions would be as represented in figure 2.

In practice we may take any star that happens to be *near* the meridian and equator, and as we cannot induce it to remain stationary we must allow for the difference of time between each observation.

So much for the *principle* of adjustment. *My usual practice* in making this adjustment is as follows:—

I choose any star that happens to be near the meridian and equator, and tolerably easily recognized, and I set telescope on this star (telescope being at E. side of pier); I start clock, and with fine motion bring star to centre of field, where clock keeps it for a few minutes if in tolerable adjustment. At any definite time, preferably an even minute, by a clock or watch (an ordinary watch will answer for this observation) I stop equatoreal clock, or disconnect it from driving-circle, and then read R.A. circle and call this reading A. I now reverse telescope to W. of pier, reset on star, start clock, bring star to centre of field, and again at any other definite minute stop clock and read R.A. circle, and call this reading B. The difference between the readings of the R.A. circle should be equal to the difference of time shown by watch or clock, plus 12 hours. If the difference of readings of R.A. circle be in excess of the difference of time as shown by watch, plus 12 hours, the angle A D C, figure 1, is too great, and *vice versâ*.

Fig. 2.



As Example	Time by Watch.			R.A. Circle.		
	h.	m.	s.	h.	m.	s.
A star is observed at E. of pier at	8	5	0	3	7	16
„ W. „	8	10	0	15	12	45
Difference	0	5	0	12	5	29
Error	..		..	..	+	29 sec.

As this adjustment is supposed to be completed in the workshop there is generally no provision made in the mounting for effecting it, and it is very seldom required, but, if necessary, a liner of paper under one or other end of cradle will suffice. In the case above cited the liner should be placed under upper end of cradle, and if it be desired to find out about the thickness required, it may be done thus. Let S = number of seconds of time of error and L length of cradle in inches,

$$\frac{15\ \mathrm{S}\ \mathrm{L}}{206265} = \text{the thickness of paper required in parts of an inch.}$$

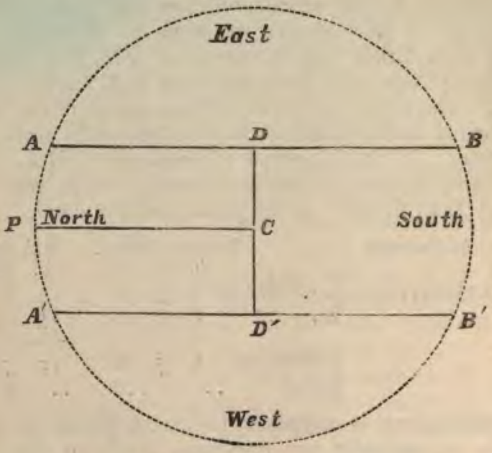
Thus, in above case, suppose cradle to be 10 in. long:

$$\frac{15 \times 29 \times 10}{206265} = \tfrac{1}{47}\text{th of an inch.}$$

The object of choosing a star on Meridian and Equator is to render this adjustment independent of others that follow after.

3. *To adjust Declination Axis at Right Angles to Polar Axis.*—This is also an adjustment which is supposed to be left sufficiently perfect from the workshop, and for which provision is only made in the larger classes of instruments. The usual mode of adjustment recommended is to hang a level on some portion of the axis specially prepared for it, and then reversing, take the readings of the R.A. circle in each case, the difference between which should be exactly = 180°. In high latitudes this plan answers well. As, however, but few modern equatoreals are now supplied with levels to declination axis, I prefer to give here a mode of adjustment which can be adopted without this apparatus. My practice is to take a star near to the Pole and proceed to observe in same way as per last (second) adjustment. If the second adjustment has been made correctly, any error found in this way must be an error between the declination and polar axis.

Fig. 3.



Suppose as before A B, Fig. 3, to be Telescope at E. of pier, and A′ B′ W. of pier; P C to be polar axis; C D declination axis, telescope E. of pier; C D′ declination axis, telescope W. of pier. Now if the angles A D C and B D C have been already adjusted to 90° by second adjustment as above, and that in observing Polar stars it is found that the two positions of telescope are not parallel when polar axis is turned through 180°, it is evident that the angle P C D is not a right angle.

All the remarks made on adjustment 2 apply equally to this, and a little consideration will show that even if considerably out of adjustment in this last direction the error could not have affected the accuracy of the observation made for No. 2, provided the star in No. 2 adjustment was close or near to the Equator.

4 and 5. *Zero of Declination Circle and Inclination of Polar Axis.*—I take these two adjustments together because the one pair of observations determines the error of both. I choose a star (again preferably as near as possible to Meridian, but higher than Equator, near to Zenith), some star whose R.A. and declination can be found in the 'Nautical Almanac' or other Catalogue; set telescope on it carefully, paying more attention this time to the setting in declination than the setting in R.A. Having satisfied myself of the setting in declination, telescope being at E. of pier, I read declination-circle. Again set with telescope W. of pier and read declination-circle; call first reading A and second B.

If there be no difference in the readings A and B the declination-circle has no zero error. If there be a difference (which is most likely) the declination-circle should be moved round (or its verniers moved) by a quantity equal to *half the difference*, and it will then be found to read the same declination for the same star, whether telescope be at E. or W. of pier: *this corrects No.* 4. *Now to correct No.* 5. The reading A or B, if they be equal (or A plus B divided by 2 if they be unequal), is the declination of that star as measured by the instrument *with its present inclination* of polar axis. Look to Catalogue, and the observed declination as above should equal the declination as given in Catalogue, plus or minus the refraction according as the star is south or north of the zenith. If the declination be a north declination and reading by instrument too high, the polar axis is too much inclined to horizon, and *vice versâ*. The best practical way to adjust this is—if it be N. declination set the declination-circle of the telescope to read the declination of the star, as given in Catalogue, plus the refraction, and then if telescope is found to point too high or too low, adjust polar axis by whatever screws are supplied for the purpose till the star reaches the centre of the field. To find out the quantity to be added for refraction; *the refraction tables give refraction for any given altitude*, and when the star is on the Meridian its altitude is equal to the co-latitude of place, plus declination if it be N. declination, or minus declination if it be S. declination. If star be chosen near Zenith the refraction may be neglected.

Another method of working adjustments 4 and 5 is as follows, but this I have never tried myself. Place the telescope vertically over a vessel of mercury, pointing downwards, and make the reflected image of cross-wires coincide with the cross-wires themselves, then the telescope points exactly to the nadir and the declination-circle should read 180° plus latitude; repeat this at the other side of the pier, and adjust the circle as directed in the case of a star. Then set the circle to 180° plus latitude and raise or lower the polar axis until the cross-wires and image coincide and the axis is right. For the Nadir observation it is necessary to throw light down the tube; the simplest way is to have a small blank eyepiece (to be put over the real one) furnished with a small piece of glass, which can be given any inclination to the horizon so as to reflect daylight or artificial light down the tube. In the case of a reflecting telescope point it to the Zenith and put a level on a straight edge across the speculum in the Meridian.

Dr. Dreyer, of Armagh, tells me he has used this latter plan in the case of Lord Rosse's 3-ft. reflector.

6. *To adjust Polar Axis into Meridian.*—This is generally a troublesome observation to effect with precision, because it necessitates the observation of stars off the Meridian, in which case the influence of refraction must be found by calculation; of course to a practical astronomer this is no trouble, but to an amateur, or indeed any one (when, as is often the case, there are plenty of clouds drifting about so that the operation must be done quickly or not at all), a method of making this adjustment without the necessity of estimating the refraction is generally to be preferred.

If there were no refractions to trouble us, and that all the preceding adjustments had been made, leaving only this one to be effected (the reading of the R.A. circle is not mixed up in this observation), it is evident that all that would be necessary would be to set on any known star some hours off Meridian (if possible at an hour-angle of six hours), and if the declination of this star did not read rightly according to the Catalogue to *make it read rightly* by moving the whole instrument in azimuth. As this is the only adjustment not perfected before, it is clear that this would be the correct course. This plan suffices for very small instruments, but for large, and where refraction must be allowed for, I take two stars, each about an equal hour-angle, one to E. and one to W. of Meridian and of about equal declinations, and I take alternate observations. When I find that the excess of the actual reading of declination over the Catalogue reading is equal with each star, I consider the adjustment to be correct, and I check this still further by taking an observation of the polar star after the next adjustment.

7. *To adjust R.A. Circle.*—For this adjustment either a level on declination axis, a mercury trough, or a knowledge of exact sidereal time is necessary. From Catalogue ascertain the exact R.A. of any star (preferably near Equator at the time of observation as the apparent motion is most rapid). The reading of R.A. circle when telescope is pointed to that star should be sidereal time, minus R.A. of star. The best practical way of making this observation is as follows:—I subtract the R.A. as found in Catalogue

from actual time shown by sidereal clock, not at that moment, but from some definite minute a little in advance of what the clock shows (to give time for setting telescope); I now set telescope on star and start equatoreal clock and see that it keeps star in centre of field: the moment the clock points to that minute for which I made my calculation I stop equatoreal clock and read R.A. circle; if it is not correct I loosen nuts holding circle and move round circle till it reads correctly. If the telescope be provided with a level to declination axis it is not necessary even to have sidereal time, for if the declination axis be levelled by the spirit-bubble and the R.A. circle be turned round till it reads XII or XXIV, this last adjustment is completed, or the verticality of telescope and consequent horizontality of dec. axis may be found by using a mercury trough as before.

I go over all these adjustments a second time and then check the azimuth position by taking an observation of the Pole star, which, moving as it does in a very small circle round Pole, shows a very large error in hour-angle for any given error in azimuth of polar axis. If necessary I adjust the azimuth once more till R.A. circle reads correctly on Pole star. If this observation of Pole star be made when it is crossing or nearly crossing the Meridian, either in its upper or lower transit, refraction would only affect the declination readings and not the R.A. readings.

Respecting the means of effecting these various adjustments, it is not easy to be precise, for different makers supply more or less adjusting arrangements, nor do they all act in the same way.

For the 1st Adjustment.—The wire plate is made generally adjustable as described.

For the 2nd Adjustment mentioned above, provision is rarely, if ever made, and if necessary it must be done, as I said, by a liner of paper or card.

For the 3rd Adjustment provision is also rarely made. In my large instruments I make the small bearings of declination axis on a slide and have opposing screws for this adjustment.

For the 4th Adjustment.—If error be large the whole circle must be turned round on its axis and again clamped by whatever provision is left for the purpose by the maker. If the quantity be small, as it is likely, the vernier will probably move a sufficient quantity if its screw be loosened.

For the 5th Adjustment.—In Messrs. Cooke's instruments and in my portable forms this adjustment is made by loosening the screws which fasten the polar axis frame to head of pillar, and working the right and left-handed screws at south end of polar frame. In my standard form this adjustment is effected in the frame between the wedge-shape casting and top plate by screws specially provided for the purpose.

For the 6th Adjustment.—In the pillar (portable form) and also in the standard instruments, provision is made for this adjustment by a right and left-handed screw.

For the 7th Adjustment.—As in the case of No. 4, if error be large the circle must be loosened on axis and turned round; if error be small, the amount of play left in the screw-holes of vernier may be sufficient. HOWARD GRUBB.

[To be continued.]

The Cordoba Zone-Catalogue *.

WITHIN the last ten or twelve years our means of determining with accuracy the positions of stars in the southern hemisphere have been increased many fold. The Cape and Melbourne Observatories have contributed largely to this very desirable end—especially may be mentioned in this connection the Cape Catalogue for 1880, containing the places of more than 12,000 stars situated between 25° south declination and the south pole, a contribution to sidereal astronomy which is due to the vigour and energy with which Mr. Stone discharged the duties of Director of the Cape Observatory, during the time in which he held the appointment of H.M. Astronomer at the Cape. The work now before us is of a somewhat different character. Dr. B. A. Gould has, after nearly thirteen years of assiduous toil, brought his zone work at Cordoba to a successful termination, and has thus been enabled to publish a catalogue containing a far larger number of stars than any previous catalogue of southern stars. It was for the sake of this series of observations that Dr. Gould originally planned a private expedition to South America, which was adopted by the Argentine nation and made the germ of its National Observatory. And although the delays attending its commencement permitted the preparation of the 'Uranometria Argentina' (a most valuable work, for which Dr. Gould was awarded the gold medal of the Royal Astronomical Society in 1883), and although opportunity has also been found for the observations from which a general catalogue has been formed and nearly completed, still the Zone-Catalogue has always been considered as of paramount importance and other work as only subsidiary to it.

The series of zone-observations was commenced in September 1872, and not finally completed until June 1883, the total number of observations being 105,240, giving the positions of 73,160 stars, so that the mean number of observations for each star is 1·44. The observations were made with a reversible transit-circle constructed by the Repsolds, the aperture of the object-glass being 121·9 mm., and its focal length 1·463 metres. The diameter of the graduated circle is 716 mm., and it is read by four microscopes. In the zone-observations it has been usual to take the transits of stars over three threads, and to read the circle by one microscope only, the difference between this reading and that of the mean of four microscopes having been determined every day. But Dr. Gould has endeavoured to obtain so-called "absolute" determinations for all the stars observed, the zenith-distances depending on nadir observations, and the right ascensions on Dr. Gould's "Standard Places of Fundamental Stars" published by the U.S. Coast-Survey. Every care has been taken to determine the instrumental errors with accuracy. All the reductions and computations of every kind were made independently in duplicate. And it was not until August 1882 that these laborious processes were completed, although no efforts were spared to push the work on with vigour. Dr. Gould alludes feelingly to the extreme difficulty of finding trustworthy persons to perform the simple arithmetical processes, and expresses his sense of obligation to the Assistant Astronomers and especially to Dr. Bachmann, who devoted himself to the examination of the zone computations and the scrutiny of the original observations. In the latter end of 1882 the work appeared to be completed and only to need copying for the press. It was found, however, that of the 73,000 stars contained in the zones, there were more than 35,000 which had only been observed once, and therefore for whose positions no control existed. Recognizing the danger of numerous errors in the positions of these stars, the four Assistants spontaneously volunteered to revise the positions of all stars for which only one observation existed, and to complete the revision in season to avoid any essential delay in printing the Catalogue. After some hesitation, Dr. Gould resolved to accept their generous offer, and accordingly the revision was begun in November 1882, and completed in June 1883, thus adding greatly to the value of the Catalogue. The gentlemen who undertook this labour were Messrs. Thome, Davis, Stevens, and Wiggin. During the progress of the revision there were 107 cases in which no star was found, either in the Catalogue place or in any position which this place would give under any reasonable supposition of error in the record. These stars have been searched for with the large equatoreal, but none have been thus discovered. There is, however, one star, to which the name *R Indi* has been affixed, which was found to be missing during the revision, but has since been duly observed in the Catalogue place, its variability being thus fully demonstrated. And Dr. Gould thinks that variability or motion is the most probable explanation of the disappearance of the other stars also. A list of these stars is given in the Introduction, page xv.

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* Resultados del Observatorio Nacional Argentino, Vols. vii. & viii. Cordoba, 1884.

To ascertain the mean error of the observations, an investigation has been made by the employment of those stars which were observed more than twice, and from the whole mass of data there results $0^{s}\cdot062$ for the average discordance in R.A., expressed in equatoreal time, and $0''\cdot972$ for that in declination. It appears, therefore, that the observations are of a high order of accuracy, and such as to testify to the skill of the Cordoba observers.

An Appendix, containing all the data needful for an easy determination of the precessional change between 1875·0 (the epoch of the Zone-Catalogue) and any other epoch, is annexed to each volume of the Catalogue. Of course where there are such a large number of stars it would have been impossible to compute precessions and secular variations for each individual star; but the very convenient tables which are given will be found nearly as easy in application.

In this great work which Dr. Gould has brought to a successful termination with such immense labour and patience, we have a complete 'Durchmusterung' of the southern heavens down to mag. 9½ (containing also several stars of the 10th mag.) between the limits of declination −23° and −80° arranged in order of R.A., and exhibiting the results of each separate observation, the great value of which can perhaps only be fully realized by observers in the southern hemisphere, who will so frequently have occasion to refer to the Zone-Catalogue which we owe to the energy, skill, and assiduity of Dr. Gould and his Assistants. A. M. W. DOWNING.

ADDENDUM.—The following extracts from a letter to J. D. Dana from Dr. B. A. Gould, dated Cordoba, Oct. 4, 1884, have been published in 'The American Journal of Science' for December, and will doubtless be read with interest:—"After fourteen years of an expatriation which I did not at the beginning intend should cover more than three, I see the end of the vast and expanded undertaking within reach. Its object has already been essentially attained, so that it would now appear that the end of the current year will see me either on my homeward way or very near to the commencement of the journey. By this present time of writing, or very soon at any rate, you will receive four quarto volumes of star-positions; and within about a month I hope to start a meteorological volume on its way. A huge mass of similar material will be left for the occupation of my successors in the two institutions. The manuscript of seven astronomical quartos I hope to leave ready for the press; and to bring with me for publication in the northern hemisphere the General Catalogue of Southern Stars, which will complete the astronomical work."

The Variable Star U Cygni.

THE results of observations of this star which I have made since the publication of Mr. Knott's paper in the 'Observatory' of July, 1878, having shown the desirability of making a re-determination of its elements, Mr. Knott has kindly communicated to me the results of his observations of five additional maxima and three minima for discussion in connection with the earlier results, and with the results of my own observations of three maxima and three minima made since the date of the paper referred to. The combined results are as follows:—

Observed times of maxima.	Observed magnitude.	Observer.
1871, June 16	8·1	Knott.
1872, Sept. 16	7·9	Knott.
1873, Dec. 29	7·7	Birmingham.
1876, July 18	8·0	Birmingham.
1877, Oct. 21	7·8	Knott.
1878, Dec. 21	7·9	Knott and Baxendell.
1880, April 12	7·8	Knott and Baxendell.
1881, Aug. 3	8·0	Knott and Baxendell.
1882, Oct. 29	8·0	Knott.
1884, Feb. 1	7·6	Knott.

Observed times of minima.	Observed magnitude.	Observer.
1870, Oct. 20	9·4	Knott.
1872, Jan. 29	9·8	Knott.
1873, May 9	10·0	Birmingham.
1877, Mar. 15	10·6	Knott.
1878, June 16	10·7	Knott.
1879, Aug. 19	11·4	Baxendell.
1880, Dec. 9	11·6	Knott and Baxendell.
1882, Mar. 3	11·6	Knott.
1883, June 14	11·5	Knott.
1884, Sept. 21	11·1	Baxendell.

The mean period obtained by Mr. Knott from the first five maxima was 464·6 days, and the period I have obtained from the last five is 466·6 days. This result appears at first sight to show that a slight increase in the length of the period has taken place since 1877; but the mean period derived from the entire series is only 461·168 days. This apparent anomaly is, however, due to the occurrence of the short period of only 426 days between 1877, October 21, and 1878, December 21, and it cannot therefore be said that any change in the mean length of the period has actually taken place.

The epoch of maxima is 1877 October 9·57; and calculating the dates with period =461·168 days, the greatest difference between the calculated and observed times is 23·73 days, and the mean difference is 7·53 days.

The ten minima give

Period=461·457 days. Epoch=1878, May 28·85.

Calculating the dates of minima from these elements, the greatest difference between the calculated and observed dates is 21·66 days, and the mean difference 8·39 days. It is evident therefore that the period of this variable is subject to considerable irregularities, which occasionally amount to about one twentieth of its mean value.

The mean magnitude at maximum is 7·9, and range of variation 0·5. The mean magnitude at minimum is 10·8 with a range of variation amounting to 2·2 magnitudes; but it must be observed that the magnitude at minimum gradually decreased from 9·4 on October 20, 1870, to 11·6 on March 3, 1882, since which time there has been a slight increase.

A noteworthy feature in the changes of this variable is the near equality of the times of its increase and decrease of brightness, the former being 230·03 days, and the latter 231·28 days.

The Observatory, Birkdale, Southport,
1884, Dec. 12.
JOSEPH BAXENDELL.

Photometry of Saturn's Rings.

THE hope expressed by Prof. Seeliger, that his investigations on the varying brightness of Saturn's rings would direct attention to this branch of photometry*, has already borne fruit. From a recent paper† by Dr. Müller, of the Potsdam Observatory, it appears that he has been engaged since 1878 in regular photometric observations of the Saturnian system, with the view of definitely establishing the dependence of its brightness on the position of the rings. After repeated trials he gave up the hope of observing the rings alone, and hence the results published depend on the total light of the ball and rings. The observations were made with a Zöllner photometer by reducing the image in the focus of telescope to a perfectly rounded point. As the bright stars α Tauri and α Aurigæ were always in the vicinity of Saturn, they were taken as comparison stars, so that, as these were repeatedly compared *inter se*, the observations can be referred to Zöllner's standard (α Aurigæ). The results rest upon 150 measures taken between 1878 July and 1884 April. Taking for abscissæ the angle of elevation (l) of the Earth above the plane of rings, and the corresponding observed log. of brightness (log H) as ordinates, Dr. Müller deduces the following table from the mean curve:—

l.	Log H.	l.	Log H.
0	9·686	14	9·907
2	9·721	16	9·935
4	9·755	18	9·962
6	9·788	20	9·989
8	9·819	22	0·015
10	9·849	24	0·041
12	9·879	26	0·066

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* 'Observatory,' Vol. vii. p. 300.

† Astron. Nachr. No. 2631.

From which it follows that when the Earth is elevated 26° above the plane of rings, the light received from Saturn and rings is just 2·4 times that received when the Earth is in the plane; in other words, the brightness of Saturn's rings when the Earth is 26° above their plane is 58·3 per cent. of the brightness of the whole system.

Comparing these mean results with the single observations, Dr. Müller found that about the time of opposition the observed value of log H was always greater than the computed, while the difference gradually diminished, until finally the computed became the greater. This led him to construct the following table of the brightness of Saturn without the ring:—

60	days	before opposition	9·652,	after opposition	9·684
40	”	”	·671,	”	·693
20	”	”	·700,	”	·710
0	”	”	·745,	”	·745

This seems to show also that there exists a difference of 7 per cent. in log H as observed 60 days before and 60 days after opposition. This may be accounted for, in some measure, by the phases of Saturn, as well as in the difference of altitude of Saturn above the horizon at the time of observation. Dr. Müller thinks that we have here some data as to the arrangement of the particles of the rings, and also as to the weakening of light caused by their shadow. In concluding his paper, Dr. Müller compares the computed brightness of rings derived from the formulæ of Zöllner and Seeliger with the brightness as observed by himself, with the result that whilst Seeliger's formula ($\gamma \sin l$) may be said to satisfy the observations, Zöllner's theory represents them fully. T. Lewis.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Prime Meridian Conference.

Sir,—

I think everybody who is in the habit of using civil and astronomical time must be sincerely glad that we have at length arrived at some satisfactory solution of the difficulty of having to deal with two times when one is quite enough.

But my chief object in writing to you on this subject is to ask a question respecting the elements in the Nautical Almanac. At present the Sun's elements are given for apparent and mean *Noon* at Greenwich, and the Moon's meridian passage, p. iv of the month, is given in astronomical time at Greenwich. Now the question is, will this be allowed to remain as at present and given for Noon at Greenwich or altered to Midnight?

It is very important to seamen that this question should be settled officially; and as I understand the Astronomer Royal intends adopting the new method on January 1, 1885, seamen are naturally asking the question, is the Nautical Almanac to be altered and the Sun's declination, equation of time, &c. given for 0^h at Greenwich or for 12^h or Noon as at present.

Of course we know, as the Nautical Almanac is published four years in advance, it will have no effect at present: but still I think if we could get some official intimation of what is intended it would remove any doubts which may exist.

If the Nautical Almanac is left as at present, the Declination &c. would simply have to be reduced to what it would be at so many hours, minutes, and seconds before or after noon when the observations are taken, which of course everybody understands, and nothing can be simpler. But if the elements are to be given at midnight, or 0^h at Greenwich, it ought to be modified to save confusion. To sum it up in a very few words, Are we to reduce our elements to Noon or Midnight at Greenwich?

Yours very truly,
DUNCAN FORBES.

11 Hanover Buildings,
Southampton.

Twilight Fireballs from Musca.

SIR,—

The twilight fireball of November 3, 1884, observed by the Rev. S. J. Johnson as it passed slowly through the stars of Canes Venatici, evidently had its origin in the special shower south of Musca, described in the 'Monthly Notices,' Vol. xlii. pp. 34–7. The maximum probably occurs between November 1–4; it appears to have given the largest number of fireballs on November 3.

The direction of the large meteor above alluded to accords precisely with the radiant-point in Musca, and its appearance, as described by your correspondent, is also in conformity with that of many other fine meteors from the same source.

Large fireballs rarely come alone. In the present instance we find that on November 3rd last, at 9^h 15^m (Paris time), a bolide, remarkable for its brilliancy and startling effect even in the presence of the full Moon, was observed at several stations in the south of France, and at Morges on the Lake of Geneva, Switzerland. At Hendaye (Basses Pyrenees) it appeared in the E.N.E. at a "declination" of about 50°, and travelled in a direction from S.E. to N.W. At Irun it passed from south to north, leaving a luminous train and eclipsing the brightness of the Moon.

The descriptions, which do not well agree with each other, would seem to indicate that this bolide probably belonged to the same stream as that of the fireball observed by Mr. Johnson earlier on the same evening.

On November 2, 1880, at 4^h 56^m P.M., a fireball was seen from several stations near London, moving slowly in an almost horizontal path near the planet Jupiter, then in Pisces. The meteor was very brilliant, though the strong twilight greatly moderated the effect, and its path proves it to have been directed from the radiant in

Musca. It could hardly have been a Taurid, as the diverging focus of this shower was below the horizon at the time of its apparition.

On November 8, 1876, at $5^h\ 4^m$ P.M., a large fireball appeared in the twilight of the western sky. It was seen from many parts of England, and Prof. Herschel derived the approximate radiant at $53^\circ + 20^\circ$ ($\pm 5^\circ$), so that this meteor supplies another example of the brilliant character of the shower in Musca.

On November 3, 1872, at $5^h\ 30^m$ P.M., a fireball, estimated $\frac{1}{3} =$ ☽, was seen at Glasgow, traversing an arc from 10° left of α Auriga to near the N.N.W. horizon, where it disappeared behind a cloud after emitting a shower of red sparks. This fine meteor also had a direction consistent with the radiant-point of the Muscids.

Twilight fireballs were also observed on November 4, 1873, at $4^h\ 56^m$, and on November 6, 1876, at $5^h\ 45^m$ (Paris time), the latter being little less in diameter than the Moon, but their recorded paths are opposed to the idea that they belonged to the same stream as those previously mentioned. The one seen on November 6, 1876, may, however, have been a Taurid moving very slowly in a much foreshortened course near its radiant-point.

The conspicuous apparition of these several fireballs in the evening twilight of the first week in November is interesting, not only in proving the extreme richness of the shower from Musca, but as showing the necessity for further observations of a meteoric epoch now so definitely established. And this epoch derives a special character from the singular lustre of its meteors and their annually recurring visibility. That so many have been witnessed in the evening twilight has perhaps little significance, being more the result of accident than of any special distribution; but we cannot help remarking on this undue prevalence of fireballs at an hour in the evening when the chief radiant in Musca has only just risen above the E.N.E. horizon, and is therefore most unfavourably situated for the presentation of its meteors.

As to the Taurid fireball epoch, that comes later. The maximum is reached about November 19–23; but large meteors occasionally fall from Taurus much earlier in the month, and I have seen them as late as the 27th (in 1880). Thus the Earth has hardly emerged from one prominent stream of fireballs before it encounters another, equally rich, and pursuing a nearly concentric direction. Indeed, during the first half of November there is a contemporary activity of the Muscids and Taurids, though I believe that, as regards fireballs, there is a marked difference in epoch, the Muscids being unusually large and bright on November 1–4 and the Taurids on November 19–23. Slow motions and bright trains of sparks are typical features of these streams. Many large Muscids are also seen during the last half of October, notably on October 17–22. The Taurid fireballs often detonate and are therefore aerolitic in character.

Yours faithfully,

Bristol, Dec. 6, 1884.

W. F. DENNING.

The Oval Spot on Mars.

Sir,—

Please allow me to say a few words about the "Oval Spot" I observed on Mars on Feb. 27th, and described in my paper entitled "The Planet Mars in 1884," printed in 'The Observatory,' Vol. vii. p. 369, as from your remarks on this object it is obvious I have not made myself clearly understood.

The extended, narrow and diffused greyish mark corresponding to the "Oceanus Fluvius" of the map of M. Schiaparelli is always more or less visible under good definition and clearness of the atmosphere of Mars; since 1877 I have seen it very frequently and drawn it about 40 times. Although in the "Oceanus Fluvius," the oval spot observed this year is altogether different from what I have seen before in its place. I have seen it but once. It was quite distinct and considerably darker than the two faint branches of the "Oceanus Fluvius" extending on either side of this object, and also than the "Lœstrygonum" running southward. In fact it appeared quite conspicuous in this spotless region and almost isolated. The figures accompanying this paper not being very exact reproductions of my drawings, I send you a copy of the original represented on Fig. 3, which has a very close resemblance with it*.

E. L. Trouvelot.

Observatoire de Meudon, Dec. 20, 1884.

Neptune.

Sir,—

Careful photometric measures gave Nov. 29^d 9^h 44^m G.M.T. as an epoch of maximum brilliancy; but the variation was only 0·4 magn. between max. and min. The period of $7^h{\cdot}92$ observed last year has been fully confirmed.

Jamaica, Dec. 3, 1884. Maxwell Hall.

NOTES.

The Natal Observatory.—We have received Mr. Neison's Report as Superintendent of the Natal Observatory, for the year ending May 1, 1884. From the Report it appears that Mr. Neison is doing good work in Astronomy and Meteorology, as far as the instrumental means at his disposal allow him to go. The excellent 8-inch equatoreal of the Observatory is in good working order, but the weather has been so unfavourable for observation that little work has been accomplished with this instrument. Several observations have, however, been made of Pons's Comet and a few of the inner satellites of Saturn. The few fine nights that there were through the year were necessarily taken up by work with the transit instrument, with which have been observed transits of Stars, of the Sun, of the Moon's limb, of the Lunar Crater Murchison A, and observations of Zenith stars for latitude—the instrument being adapted to determinations of differences of declination. Mr. Neison's more directly utilitarian work consists in the dissemination of time throughout the Colony, a work which appears to be done in a very thorough manner. The staff of the Observatory consists of the astronomer, the assistant astronomer, and a computer, and we hope that valuable work, useful to the Colony in particular and to the science of astronomy in general, may be turned out by Mr. Neison and his staff.

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* [M. Trouvelot has sent us with the above a very beautiful copy of his drawing of Mars of 1884, Feb. 27, 7^h 45^m, which fully bears out his remarks. The Greenwich drawing of 1882, Jan. 9, 12^h 2^m, shows the same spot distinctly; but its shape appears somewhat different, chiefly, no doubt, from the effect of foreshortening.—Ed.]

THE EQUATOREAL COUDÉ.—M. Loewy, in presenting to the Academy of Sciences the results of his tests of the Equatoreal Coudé which has now been erected for two years at the Paris Observatory, divides them into five heads:—

1. Stability of mechanism.
2. Power of Separation.
3. Quantity of light conveyed to focus.
4. Facility of observation.
5. Expense of instrument and the building containing it.

M. Loewy concludes that it is equal to, if not superior to, ordinary equatoreals in stability, whilst a glance will suffice to show its great superiority as a working instrument. Its power of separation is perfectly satisfactory. It separates ω Leonis, whose components are 0″·5 apart, *i. e.* it attains the limit assigned by Foucault for objectives of 0^{m}·27, and is equal if not superior to the West Equatoreal of 0^{m}·31 aperture. That the quantity of light transmitted to focus is very little, if any, less than usual was shown by observation of stars of mag. 13·5, and also by observations of planet 244, which is of mag. 13·5.

AN OBSERVATORY IN TONQUIN.—The Hong-Kong 'Daily Press' states that the new Commander-in-Chief of the French army in Tonquin, General Brière de l'Isle, has given orders to have a Meteorological Observatory erected in Haiphong, to serve as the basis for a network of meteorological stations with which it is intended to cover eventually the whole of Annam and Tonquin, and which will be in telegraphic communication with the Hong-Kong Observatory. The French Resident in Haiphong will have charge of the erection and organization of the new observatory, the foundation of which will no doubt mark an era in our knowledge of the meteorology of the Further Indies."

The advisability of founding the new observatory was suggested to the Commander-in-Chief by Dr. Borius, Surgeon-General, who has for some time supplied a want by voluntarily making observations with his private instruments. Through the courtesy of the Superintendent of the E. E. A. and C. Telegraph Company, Dr. Borius was enabled to exchange observations twice a day with the Hong-Kong Observatory, and to receive information about typhoons from here, the necessity for which was made apparent by the disastrous typhoon of the 8th July, that ravaged Haiphong, the course of which was forecast here by the Government Astronomer, whose subsequent telegrams Dr. Borius states to have produced great effect and to have convinced the most sceptical.

NEW REDUCTION OF LACAILLE'S OBSERVATIONS.—Part I. of the Report of the Superintendent of the U.S. Coast and Geodetic Survey for the year ending 1882, June, contains, in an appendix, an account of the new reduction of Lacaille's observations by Dr. Powalky, and a report thereon by Prof. C. H. F. Peters. Dr. Powalky points out that in estimating the value of Lacaille's observations we must carefully distinguish between his "Astronomiæ Fundamenta" and his "Micrometrical Observations"—the former being, in the opinion of Dr. Powalky, almost equal in point of accuracy to those of Bradley, whilst the latter are certainly of very much less value. Prof. Peters in his report says "the new reduction of the northern stars, which admit of comparison, gives a favourable idea of Lacaille's observations; the declinations especially appear not inferior to Bradley's." The imperfection of Lacaille's solar observations is to be regretted, as the fact of Dr. Powalky having to assume the equinoctial point in congruity with Bradley's observations prevents the use of Lacaille's R.A.'s being used in research concerning the precession constant. The declinations are also to some extent dependent on Bradley. It has been generally conceded that the catalogue of Lacaille's stars published by the British Association does not do justice to the observer's skill. The labours of Dr. Powalky in preparing a catalogue of the principal stars is an important and necessary step which will prepare the way to a re-reduction of the zone observations, which will, we trust, be undertaken when the real value to be placed on the observations is generally known. The small corrections to Dr. Powalky's results, rendered necessary by Dr. Auwers's recent reduction of Bradley's observations, can be easily applied. The great interval between the observations of Lacaille and recent observations at the Cape Observatory will cause astronomers to hail with satisfaction a prospect of these observations becoming available for a discussion of the proper motions of southern stars.

THE THEORY OF ABERRATION.—In the October number of the 'Bulletin Astronomique' M. Folie has published a criticism on the late Yvon Villarceau's theory of aberration, to which the attention of astronomers has recently been directed anew by the publication of Nyrén's valuable paper on the constant of aberration. M. Folie contends that it is necessary to take account of the total resultant velocity of the place of observation in considering the phenomenon of aberration, this total velocity being the resultant of the velocity of the Earth in its orbit, the velocity of translation of the solar system, and the velocity of rotation of the place of observation. And that, whereas Villarceau holds that the coefficient of aberration may be represented by $v/(V\,.\,v')$, where v is the velocity of the Earth in its orbit, V the velocity of light, v' that of the motion of translation of the solar system, and $(V\,.\,v')$ the resultant of the two latter, the true expression is $v/(\mathrm{V}\,.\,v\,.\,v'\,.\,v'')$, where the denominator is the resultant of the velocity of light and of the total velocity of the place of observation, as explained above. M. Folie also contends that Villarceau has completely omitted in his formulæ the terms arising from the systematic aberration (*i.e.* that part of the phenomenon which depends on the motion of the solar system). The complete formulæ are :—

$$a_1 \cos \alpha_1 \cos \delta_1 = \cos \alpha \cos \delta + a \cos \mathrm{A} \cos \mathrm{D} + a' \cos \mathrm{A}' \cos \mathrm{D}' + a'' \cos \mathrm{A}'' \cos \mathrm{D}''.$$

$$a_1 \sin \alpha_1 \cos \delta_1 = \sin \alpha \cos \delta + a \sin \mathrm{A} \cos \mathrm{D} + a' \sin \mathrm{A}' \cos \mathrm{D}' + a'' \sin \mathrm{A}'' \cos \mathrm{D}''.$$

$$a_1 \sin \delta_1 = \sin \delta + a \sin \mathrm{D} + a' \sin \mathrm{D}' + a'' \sin \mathrm{D}''.$$

where α_1, δ_1 are the apparent (*i.e.* affected by aberration) co-ordinates of a star, α, δ the true co-ordinates ; A D, A′ D′, A″ D″ those of the points towards which the place of observation is moving on account respectively of the velocities which have been specified ; also $a_1 = (\mathrm{V}\,.\,v\,.\,v'\,.\,v'')/\mathrm{V}$, $a = v/\mathrm{V}$, $a' = v'/\mathrm{V}$, and $a'' = v''/\mathrm{V}$.

These complete formulæ differ from those usually employed by the term in a', which Villarceau, in common with other astronomers, has neglected, the general idea being that this term is a constant, which therefore may be treated as a correction to the mean place. But this is exact for a period of time extending over a few years only (including of course the case of annual aberration), since after the lapse of years the variations in the co-ordinates of the point towards which the Sun is moving cause variations in the co-ordinates A′ D′, which ought not to be neglected. In order to show that the complete coefficient, as defined above, appears in the correction to a star's apparent place, M. Folie, confining himself to the case of annual geocentric aberration (*i.e.* neglecting diurnal aberration), finds the formula for the change in R.A., arising from aberration, expressed in terms of the apparent co-ordinates, viz :—

$$\cot(\alpha_1 - \alpha) = \cot(\alpha_1 - \mathrm{A}) - \frac{a_1}{a} \frac{\cos \delta_1}{\cos \mathrm{D} \sin(\alpha_1 - \mathrm{A})},$$

as the true co-ordinates of the star are unknown, whereas the expression generally used is

$$\cot(\alpha_1 - \alpha) = \cot(\mathrm{A} - \alpha) + \frac{1}{a} \frac{\cos \delta}{\cos \mathrm{D} \sin(\mathrm{A} - \alpha)}.$$

In the former of these the coefficient of annual geocentric aberration is $a_1/a = v/(\mathrm{V}\,.\,v.\,v')$, which differs from Villarceau's coefficient $v/(\mathrm{V}\,.\,v')$ by the omission, in the latter, of v, the velocity of the Earth in its orbit. In the latter expression the coefficient is $1/a = v/\mathrm{V}$. But M. Folie adds, geocentric aberration is a pure abstraction, except for the poles of the Earth, and therefore (introducing the velocity of rotation of the place of observation) the complete coefficient of aberration is $v/(\mathrm{V}\,.\,v\,.\,v'\,.\,v'')$. The difference between this coefficient and the preceding value $v/(\mathrm{V}\,.\,v\,.\,v'\,.)$ is, of course, practically insignificant *.

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* For the effect of systematic aberration on the apparent place of a close polar star, see Prof. Seeliger's paper, 'Astron. Nachrichten,' No. 2610.

An editorial note to M. Folie's paper shows how, by making certain assumptions, Villarceau's theory can be deduced from the complete formulæ given above.

New Red Stars.—Mr. Barnard reports the discovery ('Sidereal Messenger,' No. 29, p. 285) of three remarkable red stars. "They are all 7th magnitude nearly, and strikingly resemble each other. One is in R.A. $5^h\ 38^m\ 43^s{\cdot}4$, Dec. $+30°\ 39'{\cdot}1$ (1884·4). Another in R.A. $18^h\ 58^m\ 12^s{\cdot}2$, Dec. $+5°\ 59'$ (1884·0). The third is somewhere in the drapery of Sagittarius, although I have not found it again. With the 5-inch telescope and a power of 30 diameters (field $1\frac{1}{4}°$) these objects resemble a drop of blood, they are so intensely red. They are more remarkable in this particular than any I know of."

Transit of Comet 1884 *b* (Barnard) over Stars.—M. Ginieis, at Saint-Pons, observed this comet on Sept. 11, when it was just passing over a star, which gave it the appearance of having a brilliant central nucleus. On Oct. 20 Mr. Barnard observed it pass centrally over an 8th or 9th magnitude star. He says ('Sidereal Messenger,' No. 29, p. 284)—"During the progress of transit, though the star shone through the comet's very centre, I was surprised at the star's continuous flickering and occasionally almost total disappearance, at the same time an 8th magnitude star in the same field was steady."

Alleged Eclipse at the Death of Nerva.—In a recent communication to us Mr. Lynn suggests, anent the alleged eclipse at the death of Nerva, that as an eclipse of the Sun did actually occur on the same day just six years earlier, it is possible that the author of the "Epitome," who was writing of an event that occurred 300 years before his time, may have noted the coincidence of days, without remarking that there was an interval of six years between the two events.

Encke's Comet.—Prof. W. Tempel, on the evening of Dec. 13, discovered a faint nebulous object, which he believes to have been Encke's Comet. It was smaller than Barnard's comet, very faint, and without any central condensation. A comparison with W.B. $22^h{\cdot}874$ gave the apparent position of the supposed comet as R.A. $22^h\ 44^m\ 25^s{\cdot}63$ and Dec. $+3°\ 44'\ 26''{\cdot}3$.

Prof. Newcomb has been appointed Professor of Mathematics and Astronomy in the Johns Hopkins University, Baltimore.

We regret to learn that Dr. Th. Brédichin has resigned his position as Director of the Moscow Observatory.

Prof. Hugo Gyldén, Director of the Stockholm Observatory, who was offered the position of Professor of Astronomy in the University of Gottingen, has, in consequence of a liberal offer from the King of Sweden, decided to remain at his present post.

Minor Planet No. 242 has received the name of Kriemhilda.

Approximate Times of Transit of the great Red Spot and Equatoreal White Spot across the Central Meridian of Jupiter in January 1885.

Day of Month.	Red Spot on C.M.	White Spot on C.M.	Day of Month.	Red Spot on C.M.	White Spot on C.M.
	h	h		h	h
1.	12·6	15·0	17.	15·7	14·2
2.	18·3	10·7	18.	11·5	9·8
3.	14·2	16·1	19.	17·3	15·2
4.	10·1	11·8	20.	13·2	10·9
5.	15·8	17·3	21.	18·9	16·5
6.	11·7	12·9	22.	14·8	12·1
7.	17·4	8·5	23.	10·6	7·7
8.	13·3	14·0	24.	16·4	13·2
9.	19·1	9·7	25.	12·3	8·9
10.	14·9	15·1	26.	18·1	14·3
11.	10·8	10·8	27.	13·9	10·0
12.	16·5	16·3	28.	9·8	15·5
13.	12·4	11·9	29.	15·5	11·1
14.	18·2	17·6	30.	11·4	16·7
15.	14·1	13·1	31.	17·2	12·3
16.	9·9	8·7	Feb. 1.	13·1	7·9

The red spot is still faintly visible as an indefinite, dusky appearance, situated to the S. of the depression in the great southern dark belt. This depression seems, however, less conspicuous than formerly, and is evidently gradually smoothing off into the even tenor of the margin of the belt.

The red spot has been observed here on the C.M. as follows :—

		h m	Longitude.
1884, October	3........	18 19	233°·4
,,	8........	17 26	272 ·1
November	18........	16 16	238 ·7
,,	27........	18 36	309 ·1
December	20........	12 40	134 ·7

The equatoreal white spot appears to be greatly in advance of its time. It passed the C.M. on Dec. 18 at $16^{h}\ 58^{m}$ (Long. 335°·2), and on Dec. 20 at $18^{h}\ 6^{m}$ (Long. 333°·5). Relatively to Mr. Marth's ephemeris ('Monthly Notices,' Vol. xliv. pp. 454–458) it is now 43·4 min. = 26°·5 of Jovian longitude in front of its computed place. Its motion has been greatly accelerated, and its appearance is more brilliant than ever. The times in the above table are based on my observations from Nov. 27 to Dec. 20, 1884, which give a period of $9^{h}\ 49^{m}\ 28^{s}{\cdot}9$; but the disk of the planet should also be very carefully examined near the times given by Mr. Marth in the 'Notices' above referred to.

The longitudes in each case are computed on the daily rate

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS(East).		h
Jan.	7	13·7
	8	12·3
	9	10·9
	10	9·5
	11	8·1
	12	6·7
	13	5·3
	24	12·7
	25	11·3
	26	9·9

MIMAS(West).		
Jan.	1	10·8
	2	9·4
	3	8·0
	4	6·6
	5	5·2
	16	12·6
	17	11·2
	18	9·8
	19	8·4
	20	7·0
	21	5·6

ENCELADUS.		h
Jan.	1	6·3
	2	15·2
	4	0·1
	5	8·9
	6	17·8
	8	2·7
	9	11·6
	10	20·4
	12	5·3
	13	14·2
	14	23·1
	16	8·0
	17	16·8
	19	1·7
	20	10·6
	21	19·4
	23	4·3
	24	13·2
	25	22·1
	27	7·0
	28	15·9
	30	0·8
	31	9·7

TETHYS.		h
Jan.	2	10·4
	4	7·7
	6	5·0
	8	2·3
	9	23·6
	11	20·9
	13	18·2
	15	15·5
	17	12·7
	19	10·0
	21	7·3
	23	4·6
	25	1·9
	26	23·2
	28	20·5
	30	17·8

TITAN.		
Jan.	4	0·9
	19	22·6

HYPERION.		
Jan.	9	7·8
	30	12·8

DIONE.		h
Jan.	1	8·3
	4	2·0
	6	19·6
	9	13·3
	12	6·9
	15	0·6
	17	18·2
	20	11·9
	23	5·5
	25	23·2
	28	16·7
	31	10·4

RHEA.		
Jan.	3	16·7
	8	5·1
	12	17·4
	17	5·8
	21	18·1
	26	6·5
	30	18·7

Apparent Elements of the Ring.

Greenwich Noon.	Position-angle of Minor Axis.	Latitude above plane of Ring.	
		Earth.	Sun.
1885, Jan. 8....	354° 50′	26° 44′ S.	26° 49′ S.
28....	354 57	26 49 S.	26 49 S.

Greenwich Noon.	Outer Ring.		Inner Ring.		Semi-maj. axis of Orb. of Titan.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	
1885, Jan. 8..	45″·93	20″·66	30″·54	13″·74	206″·8
28..	44 ·75	20 ·18	29 ·75	13 ·42	202 ·0

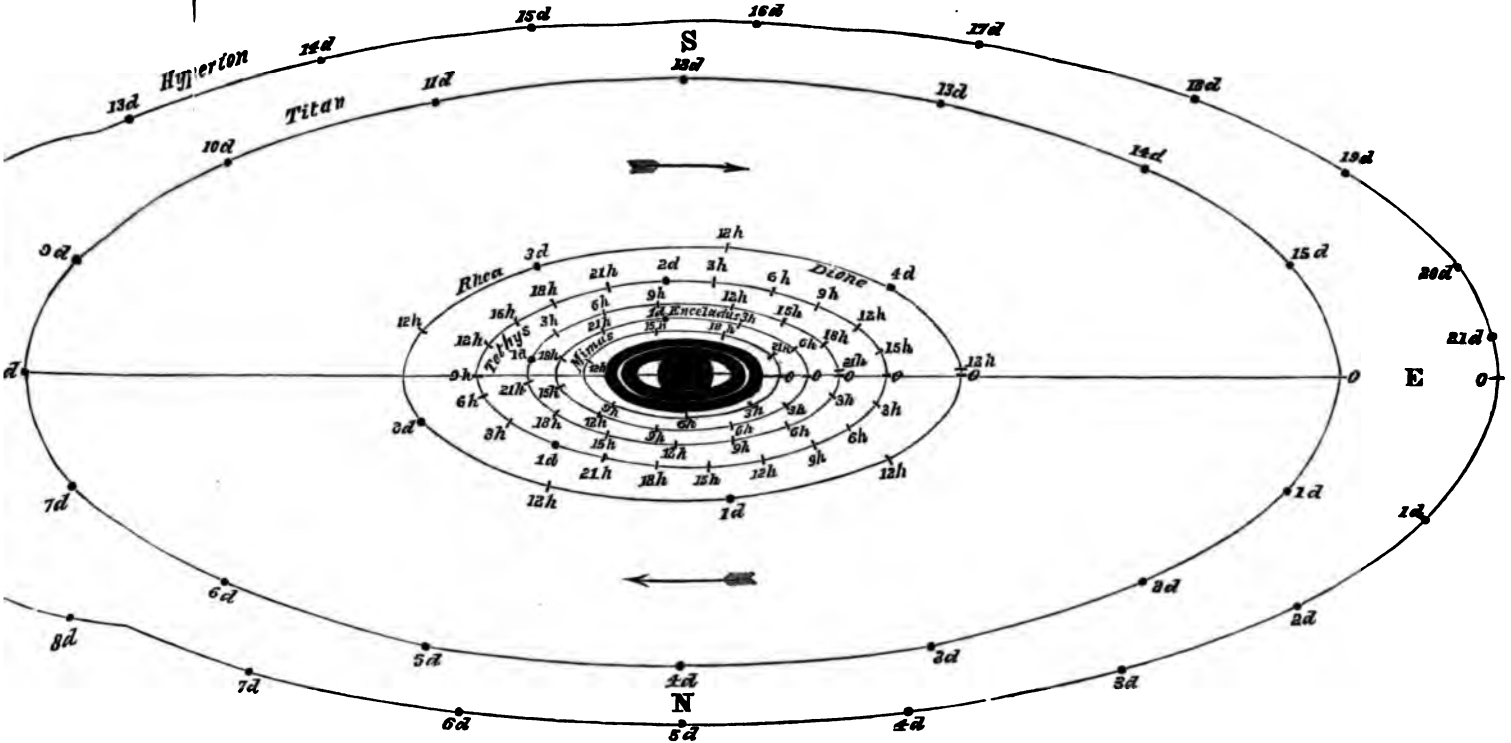
The semi-major axis of the orbit of Titan is given as well as the position-angle of the minor axis of the ring, in order to furnish means for roughly inferring the distances and position-angles of the satellites from the diagram given in Plate I.

Astronomical Memoranda, January 1885.

Equation of Time:—Sun *after* Clock, Jan. 1, $4^{m}\ 0^{s}$; Jan. 11, $8^{m}\ 20^{s}$; Jan. 21, $11^{m}\ 42^{s}$; Jan. 31, $13^{m}\ 45^{s}$.

Sidereal Time at Mean Noon:—Jan. 1, $18^{h}\ 45^{m}\ 12^{s}$; Jan. 11, $19^{h}\ 24^{m}\ 38^{s}$; Jan. 21, $20^{h}\ 4^{m}\ 4^{s}$; Jan. 31, $20^{h}\ 43^{m}\ 29^{s}$.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East elongation.

$19^{h}\ 24^{m}\ 38^{s}$; Jan. 21, $20^{h}\ 4^{m}\ 4^{s}$; Jan. 31, $20^{h}\ 43^{m}\ 29^{s}$.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
Jan. 1	20 8	3 59	1 35	−3 19	337 12
6	20 7	4 6	359 8	−3 53	271 22
11	20 4	4 12	356 44	−4 24	205 30
16	20 0	4 19	354 21	−4 54	139 40
21	19 54	4 27	352 3	−5 21	73 50
26	19 48	4 37	349 51	−5 46	8 1
31	19 41	4 45	347 44	−6 8	302 12

The Moon.

	rises. h	m		*rises.* h	m		*sets.* h	m		*sets.* h	m
Jan. 1	5	1	Jan. 9	14	31	Jan. 17	6	0	Jan. 25	15	13
2	6	16	10	15	32	18	7	4	26	16	21
3	7	34	11	16	30	19	8	10	27	17	23
4	8	50	12	17	24	20	9	17	28	18	17
5	10	4	13	18	12	21	10	26	29	*rises.*	
6	11	14	14	18	55	22	11	37	30	5	5
7	12	22	15	*sets.*		23	12	49	31	6	21
8	13	28	16	4	59	24	14	1			

Jan. 7^{d} 15^{h} 36^{m} Last Quarter. Jan. 15^{d} 20^{h} 36^{m} New Moon.
23 13 26 First Quarter. 30 4 19 Full Moon.

Planets for January 1885.

Day.	Planet.	R.A.	Dec.	Diam.	Rises.	Tr.	Sets.
		h m s	° ′	″	h m	h m	h m
1	Mercury..	19 12 23	20 18 S.	9·8	20 14	0 27	4 40
11		18 24 59	20 2 S.	9·2	18 40	22 55	3 10
21		18 32 30	21 20 S.	7·4	18 21	22 28	2 35
31		19 15 34	22 4 S.	6·2	18 31	22 33	2 35
1	Venus ..	16 40 0	20 52 S.	12·0	17 47	21 56	2 5
11		17 33 3	22 25 S.	11·6	18 10	22 10	2 10
21		18 26 58	22 52 S.	11·3	18 27	22 24	2 21
31		19 20 51	22 10 S.	11·0	18 36	22 39	2 42
1	Jupiter ..	10 31 25	10 28 N.	39·0	8 46	15 43	22 40
11		10 29 31	10 42 N.	39·9	8 4	15 2	22 0
21		10 26 30	11 2 N.	40·7	7 20	14 20	21 20
31		10 22 34	11 27 N.	41·4	6 34	13 36	20 38
1	Saturn ..	5 13 18	21 35 N.	18·4	2 22	10 26	18 30
31		5 6 21	21 32 N.	17·8	0 17	8 21	16 25
1	Uranus ..	12 11 55	0 29 S.	4·0	11 22	17 24	23 26
31		12 10 59	0 21 S.		9 23	15 25	21 27
1	Neptune .	3 15 16	16 14 N.		0 58	8 28	15 58
31		3 14 18	16 12 N.		23 1	6 30	13 59

Mercury—at inferior conjunction Jan. $3^d\ 10^h$; stationary $14^d\ 8^h$; at greatest elongation (24° 51′ W.) $26^d\ 4^h$.

Uranus—stationary Jan. $6^d\ 17^h$.

Neptune—stationary Jan. $30^d\ 7^h$.

Phenomena of Jupiter's Satellites, January 1885.

d	h	m	
1	16	16	i. Tr. I.
	18	35	i. Tr. E.
2	12	26	ii. Ec. D.
	12	32	i. Ec. D.
	15	49	i. Oc. R.
	17	22	ii. Oc. R.
3	10	43	i. Tr. I.
	13	2	i. Tr. E.
4	10	15	i. Oc. R.
	11	5	iii. Ec. R.
	11	25	iii. Oc. D.
	11	55	ii. Tr. E.
	15	0	iii. Oc. R.
8	14	12	iv. Tr. I.
	18	3	i. Tr. I.
	18	43	iv. Tr. E.
9	14	25	i. Ec. D.
	15	2	ii. Ec. D.
	17	35	i. Oc. R.
10	12	30	i. Tr. I.
10	14	49	i. Tr. E.
11	11	19	ii. Tr. I.
	11	32	iii. Ec. D.
	12	2	i. Oc. R.
	14	15	ii. Tr. E.
	18	30	iii. Oc. R.
12	9	16	i. Tr. E.
13	8	55	ii. Oc. R.
16	16	8	iv. Ec. D.
	16	18	i. Ec. D.
	17	39	ii. Ec. D.
17	14	16	i. Tr. I.
	16	35	i. Tr. E.
18	10	46	i. Ec. D.
	13	36	ii. Tr. I.
	13	47	i. Oc. R.
	15	29	iii. Ec. D.
	16	32	ii. Tr. E.
19	8	42	i. Tr. I.
	11	2	i. Tr. E.
20	11	15	ii. Oc. R.
22	11	33	iii. Tr. E.
23	18	11	i. Ec. D.
24	16	1	i. Tr. I.
	18	21	i. Tr. E.
25	9	40	iv. Tr. E.
	12	40	i. Ec. D.
	15	31	i. Oc. R.
	15	52	ii. Tr. I.
	18	47	ii. Tr. E.
26	10	28	i. Tr. I.
	12	47	i. Tr. E.
27	9	33	ii. Ec. D.
	9	58	i. Oc. R.
	13	33	ii. Oc. R.
29	7	54	ii. Tr. E.
	11	19	iii. Tr. I.
	14	54	iii. Tr. E.
31	17	46	i. Tr. I.

Occultations, January 1885.

Day.	Star.	Mag.	Disap.	Angle.	Reap.	Angle.
			h m	°	h m	°
2	B.A.C. 2872	6	7 10	22	7 36	322
2	α Cancri........	4	18 57	67	19 52	260
4	35 Sextantis	6	14 46	75	16 0	237
21	B.A.C. 57	6½	5 39	126	6 52	281
22	*e* Piscium	5½	5 33	104	6 47	302
26	B.A.C. 1526	6	7 13	38	7 49	338
27	B.A.C. 1930	6½	8 38	46	9 30	315
28	λ Geminorum ..	3½	14 23	94	15 21	244
30	B.A.C. 3122	6½	8 54	66	9 59	266
31	π Leonis	5	5 53	41	6 35	293
31	B.A.C. 3529	6	16 16	60	17 20	256

The R.A. and N.P.D. of Planets and the Solar Co-ordinates are given for Greenwich Mean Noon; in all cases Greenwich Mean Time is used, reckoning from noon to noon, as in previous years; the angles for Occultations are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 94. FEBRUARY. 1885.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

January 9, 1885.

E. Dunkin, F.R.S., *President*, in the Chair.

Secretaries: E. B. Knobel, Esq., and
Lieut.-Col. G. L. Tupman.

The Minutes of the last Meeting were read and confirmed.

The thanks of the Society were unanimously voted to the respective donors of the 52 presents announced.

The President. I have to inform you that the Council have this day awarded the Medal of the Society to Dr. Huggins, for his observations of the motion of stars in the line of sight, and for his photographic spectra of stars; and I am sure you will all second the Council in their decision. (Applause.)

Mr. Knobel explained that, partly in consequence of the length of the papers, most of which had to be corrected by the Assistant-Secretary, and partly by the necessity of re-cutting one of the woodcuts, the 'Notices' would not be ready for some days.

Mr. Spitta read a note on an observation made by him on Nov. 23. While examining Saturn with a telescope of 10 inches aperture, his attention was drawn to a point of light near the inner edge of the ring. Applying his occulting eyepiece, he found that the brightness was unmistakable. He could not say how long it lasted, but probably 1½ minute. He could not say that it was a star, but Mr. Bryant had ascertained that there are 4 stars which might have been occulted by the planet: one of these was of the eleventh magnitude.

Mr. Ranyard. It could not have been a satellite, with the rings in their present position.

Mr. Stone read a paper on "Screw-wear, as affecting the N.P.D.s of the Cape Catalogue for 1880." With regard to Mr. Gill's statements as to serious systematic screw-errors in the north polar distances of the Cape Catalogue for 1880, Mr. Stone said that it was much easier to say that there was a relative wearing of the screws between 1856 and 1879 than to prove it. It was perfectly true that two surfaces which rub must to some extent wear; but the question whether the relative wear was sufficient to lead to serious errors could only be settled by examining the facts of the case. He had directly examined this question for the Cape Circle both in regard to the screws as a whole and taking different parts of the screws. The results of the observations of the nadir points at 0 revs. and 2 revs. in 1875, as printed in the Cape Results for 1875 and for 1877, showed that, so long as the observation was confined to the five or six threads of the screws in constant use, the defects of the screws were confined within narrow limits, but that the employment of one or two threads outside the necessary range soon brought relative errors into existence. Such rapid increase of errors showed that it is utterly useless to compare errors of screws at different times, unless it were certain that no change had taken place in the adjustment of the index from which the revolutions were taken. Adjustments were, as a matter of fact, made from time to time, and that was probably the explanation of some of Mr. Gill's results. The Cape Transit Circle was used from 1856 to 1860, when the screws were new, and a considerable number of observations have been made of stars, which were regularly observed at Greenwich since Bradley's time. He had compared the places of these Bradley stars in north polar distance, as given in the Cape Catalogue for 1880, with the Cape Catalogue for 1860, brought up to the epoch 1880, with Peters's constant of precession and adopted proper motions. There are 92 stars for comparison between the two Catalogues, and he found there were only two cases in which the actual difference amounted to 1″. He was quite aware that no instrument was perfectly independent of systematic errors; but he felt perfectly sure there were no such large systematic errors of the kind that Mr. Gill had indicated in existence in the Cape Results. Mr. Gill had given also the difference between the right ascensions of the Cape Catalogue for 1850 and 1880 when reduced to a common epoch, and the difference came out about $0^{s}{\cdot}06$; but there was no material on which to make an accurate determination of the proper motions of Southern stars. They could not bring up the places for twenty years with sufficient accuracy to justify any attempt to base on such differences instrumental systematic errors. He had compared the Catalogue-places between N.P.D. 124° and 170° with the Melbourne Results for 1875, but instead of a large difference he found it $+0^{s}{\cdot}02$. He hoped Mr. Gill would not attempt to apply empirical corrections to his results. There were considerable differences, quite as large as Mr. Gill stated, between the system of clock-stars that were actually adopted by astronomers for their work. It would be impossible to say that within 0″·25 there might not be systematic errors; but beyond that he saw no indications of it, and it was obvious that if the individual errors from the range of screws were confined within limits of three or four tenths they could not get any large systematic errors in north polar distance.

The President. I am glad Mr. Stone has come before us with this explanation. Having examined the places of several stars in the Catalogue and found them correct, I was surprised to find that corrections for systematic errors, depending on the screws, were to be applied. No doubt the wearing of screws has some effect, and I do not think Mr. Stone will deny it; but I cannot agree to so large an effect as Mr. Gill supposes. I think Mr. Stone's explanation a very clear one, and that he has answered Mr. Gill fairly on the question. Great honour redounds to Mr. Stone for his Cape Catalogue, and I have no doubt it will come out unscathed from any reflections cast on it, even by Mr. Gill.

Mr. Knobel read a note on two stars in Ptolemy's Catalogue. The descriptions of the eighth star in Scorpio and the second star in Orion, as given in the 'Almagest,' are not correct. Recent translators, in describing these stars as red, have rendered the text as they thought it ought to be rather than what it actually was. He was assured that in the Arabic there was no word to express colour. He had examined the Arabic versions, and had found the following words applied to the stars:—*el husi*, *el chūsi*, *el hūd*, *el harad*, and *el harah*. These forms in Arabic were very similar. The first three could not be found in any lexicon, and were probably errors of the copyists; *el harad* meant sallowness or pallor, and also wax-like, and was derived from a word meaning fierce or fiery. He had also an explanation to give why 60° often occurred in Ptolemy where 90° was meant. Each figure was remembered in Arabic by a name, and the names for 6 and 9 were two forms of the letter *s*, so that one figure might easily be put for the other. His reason for bringing this subject forward was that Prof. Peters was engaged in the endeavour to obtain a better edition of Ptolemy's Catalogue, and that he also was assisting in this undertaking.

The President. I am sure we are much obliged to Mr. Knobel for his communication. Any one taking the trouble to investigate old archives, in the hopes of elucidating some of these points of the early history of Astronomy, is doing good service. I know that a whole day may be spent in deciphering a single line of these old manuscripts, and I am sure we must all admire the patience shown by Mr. Knobel in investigating this point.

Colonel Tupman read a paper by *Mr. Downing* on the periodic time of α Centauri. The paper was in reference to one by Mr. Powell which appeared in the 'Monthly Notices' for November last, in which that gentleman stated his belief that the period of this binary could not be less than 86 or 87 years. Mr. Downing, however, shows that a period of about 76 years would satisfy, within reasonable limits, both the observations made before Herschel's time, and also some measures made last year by Mr. Tebbutt.

Mr. Stone. Some southern observers have been using α Centauri for their instrumental corrections; a surveyor in New Zealand has found the catalogue-place considerably out. This is perfectly true, the mean proper motion of the two stars ought to be used.

Mr. Knobel exhibited a new series of graphical representations of orbits prepared by Mr. Marth; in this diagram a circular orbit in the plane of the ecliptic was represented by a dot, while a line represented an eccentric orbit in the same plane; an eccentric orbit inclined to the ecliptic is shown by a circular arc, and so on. These graphical charts were intended to serve in the investigation of close proximities.

Mr. Knobel read a note by *Mr. Franks* on Prof. Pritchard's comparison of the light transmitted by refracting and reflecting telescopes. Mr. Franks contended that Dr. Robinson was not the only authority; contributions on this subject could be found in the 'Astronomical Register' for 1867 and 1872, the 'Astronomische Nachrichten,' and also in the 'English Mechanic' for 1879. Prof. Pritchard had not fulfilled the conditions to be observed in such comparisons; different observers were employed, different wedges and eyepieces were used, and the telescopes had different foci. He could not concede that the reflective power of silver was greater than that of speculum metal, nor could he quite assent to Mr. De la Rue's statement that speculum metal reflected actinic rays better than the silver on glass, because, if that were so, how was it that speculum metal imparted a distinctly red tone?

Mr. Common. I am glad of this opportunity of stating that since Prof. Pritchard read his paper, I have carefully considered the subject, and have come to the conclusion that the lines I rather hurriedly went on were wrong; Mr. Franks appears to have trodden in the same course. The angle of incidence of the cone of rays has nothing to do with it; and whether you place the medium whereby you extinguish the light in the focus or not, is immaterial. The fact of two wedges being used does not injure the results, because their relative effect was accurately gauged and taken into consideration. The only conclusion I am able to come to as regards the results obtained by Prof. Pritchard is, that the surface of silver experimented upon was not so good as it ought to have been. My own opinions as to the relative light-transmitting powers of silver on glass, newly polished, and the best object-glasses, coincides with those of Dr. Schroeder and Prof. Zöllner. From an exhaustive series of experiments, these gentlemen came to the conclusion that the reflective power of newly deposited and well-polished silver was greater than the transmitting power of any object-glass; but after a very short exposure to the atmosphere this superiority was lost, and the two became of equal power; after this it gradually lowered, and came to an almost permanent state inferior to the ordinary object-glasses. With respect to speculum metal they found that at the outset it was inferior in reflective power to the newly deposited silver; but that after a time the two kept pretty close together, the speculum always being a little the inferior for the same length of time. I must say that having tried the wedge as used by Prof. Pritchard, I am rather a believer in it, and as regards the comparative powers of the instruments actually experimented on, we cannot doubt the result of his research; but the general question as between reflectors and refractors, I do not believe we can determine; for had the reflectors been newly polished, their power of bringing the light from a star to a focus would in all probability have been found to exceed that of the refractor. The question is worthy of discussion. I believe Prof. Pritchard's method is correct, and I cannot agree with any of the remarks made by Mr. Franks.

Mr. Ranyard. Mr. Common has omitted one element which is very important; I mean the thickness of the silver film. A great deal of light passes through a thin silver film.

Mr. Knobel. I have received a letter from Prof. Pritchard on this subject, in which he says the mirror was silvered originally 5½ years ago by With; it has since been re-silvered, and when quite fresh its power was measured—the difference being less than five per cent.

Mr. Ranyard. Perhaps the film on the second occasion was very thin.

Mr. Rand Capron. I think in experiments of this kind it would be well to substitute a prism for the flat as a second reflector; the reflective power of the prism could be easily ascertained, and it is less variable than the flat. From experience with an 8½ reflector, I also find an increase of light.

Capt. Abney. With reference to Mr. Ranyard's remark anent the thinness of film, I may say that it would affect the violet rather than the most luminous rays. Thickness or thinness of film has therefore practically very little to do with it. Mr. De la Rue's remarks at the December Meeting in regard to the actinic rays were quite correct. Perhaps I may state that, for spectroscopic work, I have done away with lenses; reflectors give a brighter image.

The following papers were also announced:—

Communicated by the *Astronomer Royal.* "Observations of Occultations of Stars by the Moon, and of phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, in the year 1884."

E. J. Stone. "Observations of the Moon made at the Radcliffe Observatory in the year 1884, and a comparison of the results with the tabular places from Hansen's Lunar Tables."

J. Gledhill. "Occultations of Stars by the Moon, and phenomena of the satellites of Jupiter and Saturn observed at Mr. E. Crossley's Observatory, Bermerside."

E. J. Spitta. "Observations of Stars occulted by the Moon during the Eclipse of 1884, Oct. 4."

Rev. A. Müller. "Observations of Comet *c* 1884 (Wolf) made at Stonyhurst College with the 8-in. telescope."

The following gentlemen were elected Fellows of the Society:— Lieut. W. St. L. Chase; Latimer Clark; W. Goodacre; G. Hamilton; Rev. A. Henderson; W. J. Ibbetson, B.A.; Rev. G. W. James; Asutosh Mukhopadyay; Prof. K. D. Macgamvala, M.A.; Thos. C. Porter, B.A.; Capt. B. Thomson, R.N.R.; H. H. Turner, B.A.; J. Wigglesworth.

The Meeting adjourned at 10 o'clock, P.M.

Liverpool Astronomical Society.

THE fourth Meeting of the Session was held on the 12th January, the President occupying the chair. Mr. W. H. Gage, F.R.A.S., submitted a list of coloured stars from the constellation of Leo. He had also prepared a list of the colours as they appeared during bright moonlight. Exception had been taken to some remarks he had made in his last paper, and he thought it would render his meaning clearer by explaining that his object had been to describe these colours with as little ambiguity as possible, because, if any law existed as to the distribution of colour, it would be likely to escape detection by employing a multiplicity of terms. It was often difficult to describe the exact tint of a star, and he would, for example, class a star as "white, lilac tinge," when the star on the whole was white with an occasional flash of lilac.

Mr. W. S. Franks, F.R.A.S., said it was stated, in a paper read before the Society last month, that it was doubtful if there was any really *red* star in the sky; but, whilst agreeing that many reputed red stars were merely orange, he would contend for the existence of stars which were indisputably red. Only the other evening he came across B 521 Cygni, which was like a drop of blood on the black sky. In colour-observations so much depended on aperture, and it was scarcely necessary to point out that a small refractor was wholly inadequate to deal with them. His own experience confirmed that of Huggins, who found colour imperceptible with either too much or too little light. Evidently, then, in any comparisons of the results of different observers due regard should be paid to the apertures used by them. Variations in colour also would often lead to misconception; he had, for example, observed Mira Ceti some time ago and entered it as yellow, but during last month he found it decidedly red. The President said they fortunately had a guide to colour in photography. He had often noticed that certain stars were either below their normal magnitude or were missing altogether from the plate, and on looking up these stars with a telescope he invariably found they were red. Mr. J. E. Gore, F.R.A.S., would like to point out that Mira Ceti, alluded to by Mr. Franks, was now rapidly rising to a maximum, which promised to be exceptionally bright this year. It was already 4½ mag. and very favourably situated for observation, as it was on the meridian early in the evening.

The Rev. S. J. Johnson, M.A., F.R.A.S., called attention to the approaching occultation of Aldebaran. The first of this group of occultations happened on the 30th ult., but its limiting parallel was 55° N., so he supposed it was for this reason omitted from the list of occultations in the 'Nautical Almanac.' It would, however, be visible in Scotland, and he hoped members would send some particulars of their observations.

Mr. E. F. Sawyer, Cambridgeport, Mass., was pleased to see that Mr. J. Baxendell, Jun's., observations of R Ursa Majoris, as reported in the 'Journal' for November, fully agreed with his own, which had extended from Aug. 11 to Nov. 6, and were 32 in number. He had noticed that the brightness increased from 8·2 on the 11th August to 7·1 on the 29th; the increase therefore was very rapid and about half a magnitude more this year than last.

Mr. Thomas G. Elger, F.R.A.S., read a paper illustrative of a drawing of Aristarchus and the W. wall of Herodotus. The observation was made on the 8th March with powers of 350–400. He also described the remarkable clefts or valleys which lie between the two formations.

The meeting adjourned at $9^h\ 30^m$, P.M.

On the Adjustment of Equatoreal Telescopes.

[Continued from p. 18.]

HAVING now described the *rationale* of the various steps of the process, I proceed to put instructions into a concise form; but as the method of proceeding which would be suitable for a large first-class equatoreal would be absurdly complex for adjusting small portable instruments, and, on the other hand, instructions sufficiently precise for adjusting small portable instruments would be quite insufficient for other larger sizes, I give three different sets of instructions.

A I consider sufficient for the largest and most perfect instruments.

B is suitable for the small sizes of equatoreals, say for 4 inches and over; while

C is only applicable for very small size equatoreals, or those of portable form, which are not expected to keep in permanent adjustment.

A.—Having placed the instrument in approximate position:

1. Adjust finder to point to the same object as great telescope.

2. Observe star near Meridian and Equator, telescope being to E. of pier, clamped, and clock going. At any certain minute by watch, stop or disconnect equatoreal clock and read R.A. circle; call this A. Repeat this observation with telescope W. of pier, and stopping clock at any other minute, read R.A. circle again; call this B. If difference of reading A and B, plus or minus 12

hours, be equal to difference of time of observation as shown by watch, telescope is at right angles to declination axis. If the difference of reading A and B be greater than difference of time by watch, the angle A D C is too large, and *vice versâ*: for estimation of quantity see above.

3. Having made this right, repeat above observation, using a star near pole; any error found by this observation must be between declination and polar axis, if No. 1 has been properly corrected.

4. Choose any well-known star near Meridian and N. of Equator, say not less than 30° or 40° N. declination; set telescope on this, paying particular attention to the readings of declination-circle (telescope being E. of pier); call this A. Repeat this with telescope W. of pier; call this B. If A equals B there is no Zero error of declination-circle; if A be greater or less than B, the declination-circle has a Zero error equal to the difference A minus B divided by 2; set this and try this pair of observations over again till the declination of every star reads a similar quantity with telescope at E. and W. of pier.

5. Now set declination-circle to read whatever declination is given in Catalogue for that star, plus the refraction for its altitude (equal to co-latitude plus N. declination); and if the star is not in centre of field, alter inclination of polar axis till it is.

6. Choose two stars of about equal declination (preferably high stars) E. and W. of Meridian, each being from three to six hours from Meridian. Set declination-circle to read the proper declination for that star which is to W., omitting any correction for refraction. If star cannot be brought into centre of field with that declination setting, alter azimuth screw till it does. Now turn to star at E., set declination-circle to its proper reading for it; if it cannot be brought into centre of field with that declination reading, turn azimuth screw till it is in centre again, *counting the number of turns*; now turn back azimuth screw half that quantity and the instrument should be in adjustment. This may be checked by taking the actual readings for declination-circle for both E. and W. stars; and if about equal altitude the errors in each case (which are due to refraction) should be equal.

7. If there be a level to declination-axis, level axis by it and set circle to read XXIV or XII. If there be no level, turn telescope on any star near Meridian; and if the circle does not read correctly, *i. e.* sidereal time minus R.A. of star, cause it to do so by altering it or its vernier.

Lastly, turn on pole star, at its upper or lower transit, setting circle to the proper R.A. or declination. If pole star be not in centre of field, bring it so by altering azimuth motion of telescope a very small quantity.

B.—Less accurate set of adjustments:

1. Adjust finder.

2, 3. Omit altogether if telescope be from any good maker.

4. Same as A.
5. Same as A.
6. Same as A.
7. Same as A.

C.— Least accurate set of adjustments :

1. Adjust finder.

2, 3. Omit altogether.

4, 5. Same as A.

6. Set telescope to declination of any star a few hours E. and W. of Meridian; turn telescope on star; if it is not possible to get it into centre of field by R.A. motion alter azimuth till it is.

7. Same as A.

Adjustment of Object-Glasses in their Tubes.—It is generally taken for granted that this adjustment is made before it leaves the maker's hands; but it often happens that after a journey the counter cell into which the objective screws is found to be slightly displaced, and the objective is used, perhaps for years, in an imperfect state of adjustment in consequence of its owner not knowing how to detect any want of adjustment or correct it when detected.

To Detect want of Adjustmeut.—Direct telescope to any convenient star. If the telescope be of large aperture it is not well to use too large a star, and in every case I like to make the final adjustment on a small star; one just sufficiently brilliant to see one or two rings round it with a high power is best.

In commencing also I use a low power, but as the adjustment gets more nearly perfect I use higher powers.

Get star fairly in centre of field and focus; observe if the light round star be symmetrical. Whether it be scattered light, as it will be with low power and bad atmosphere, or in the form of rings more or less condensed, as it will be on good nights with a high power, it should be *symmetrical.* If it appears symmetrical try a higher power, and if necessary a smaller star.

I find it assists appreciation of the symmetry or want of symmetry of the image, to keep the focussing button moving in and out of focus both ways. If under any power or on any star in centre of field the image appears unsymmetrical, the object-glass requires adjustment. In every case that side of the objective at which *most flare* is perceived is *too far* from the eyepiece.

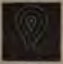
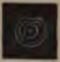
To Adjust.—Loosen all the screws which hold the counter-cell to tube, leave them just so tight as to hold the counter-cell wherever it be moved to, but not so tight as to prevent it being moved at all; now with a piece of wood like a small mallet tap the flange of the cell or counter-cell towards eyepiece at that side at which most flare is seen; no idea can be given of how much to move it, but a very small quantity will in most cases suffice, $\frac{1}{100}$th of an inch makes a very sensible difference in a moderate-sized objective.

It is very unlikely that the first attempt will make it right, but by repeated trials and tappings a satisfactory position will probably be found, and then the screws may be again tightened. It is very possible that this very tightening of the screws will again disturb the adjustment and it may be necessary to go back on it.

Fraunhofer constructed a small apparatus for testing whether the axis of the object-glass passes through the centre of the eyepiece. It consists of a small collimator in a tube which is fastened to a small plate with three screws. Place this on the object-glass, two of the screws touching the rim of the cell, and point to centre of eyepiece by the three screws, then carry the apparatus round the circumference and see if remains pointed to centre of eyepiece.

It may be asked, why do not makers supply adjusting-screws for such a delicate adjustment? I have done so in some cases, but except for very large objectives I did not find them to work well. It would appear that in the case of using screws the cell is liable to be slightly strained before it begins to move to its new position; but the very fact of the blow of the mallet shaking the whole cell causes it to jump promptly into its new position without strain. Of course in the case of very large objectives such as the Vienna, in which the glass and cell weighed some 250 lbs., a mallet to have any effect should be of considerable weight, and no one would like to have to use such a weapon in the vicinity of a £4000 glass; and here I did apply special adjusting apparatus. If it so happens that with *any* adjustment of the glass as above described symmetry is not obtainable, other matters should be looked to. It is possible (as I have sometimes found) that the eye-tube is not in line with axis of telescope. The best way to test this is to draw out slip-tube which carries eyepiece and insert in inner end a cardboard disk with a small, say $\frac{1}{10}$th inch, central hole. Now place it back again and insert a high-power eyepiece, or, better still, the eye-stop only of such in its ordinary place at outer end of slip-tube. Now look through objective at eyepiece, and travel eye about till you see light through *both* holes: then your eye *is* in axis of eye-tube, and this should be the centre of objective, which you can readily test. If it be not, the eye-tube is not square in tube, and the telescepe had better be returned to the maker for alteration.

If this eye-tube be found right, but image is still not symmetrical, something is probably wrong with objective which is beyond the power of any one but the maker to attempt to rectify. I would, however, desire to mention a few matters which may disturb this symmetry and cause what is really a good glass to be unduly condemned.

Temperature has a most extraordinary effect on the correction of an object-glass. This is not the place to discuss this fully; but I may mention that an objective taken from a warm room to open air and inserted in its tube will, after a minute or so, begin to show *under correction* for spherical aberration, which will increase sometimes to an alarming extent, the maximum being reached at a time which will vary according to circumstances (I have known it to increase for over half an hour) and then gradually decrease till it once more assumes its normal state. Again, if tube be warmed by Sun the image becomes quite unsymmetrical, the section of cone of rays being vertical outside focus and horizontal inside focus. If objective be put in warm and it warms the air inside tube the appearance is that of  outside focus and  inside. I mention this to prevent persons forming any judgment too hastily.

Personal Defects of Eyesight.—Very few eyes are really symmetrical in themselves, and the effect on the image in the telescope is very apparent.

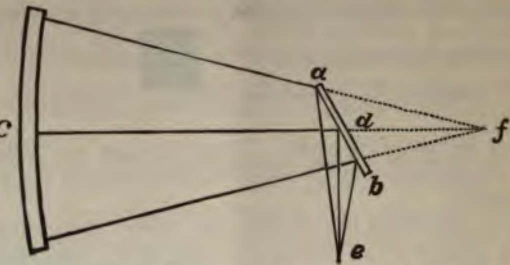
My own eyes when examined by an oculist were considered to have very little want of symmetry, very little "*astigmatism*" as it is called, so little that a 70-inch focus cylindric lens corrects this; still this astigmatism is a great trouble to me in testing objectives.

When an objective is best for my own eyes I know it is not perfect, and in final trials I always make a practice of continually altering the position of my body as respects any given diameter of the objective. This astigmatism of the eyes, however, is only apparent in using *low* powers, probably becase a larger aperture of the crystalline lens is used in that case.

In all cases of want of symmetry of image in a telescope the cause can be localized by turning round successively each part of the apparatus: first say the object-glass (if the unsymmetrical figure remains in same position it is not the objective that is in fault), then the tube, then alter the position of body, and so on. If, as I have often found, the unsymmetrical figure of image continues constant *through all these experiments*, it is probably due to layers of air of unequal refrangibility in tube. After a tube has been recently blackened on inside this is often seen very strongly, the vapour from the varnish accumulating at upper side and forming there a stratum of different density to the rest of the air in tube.

Adjustment of Mirrors in Reflecting Telescopes.—Whether the telescope be of the form of the Newtonian, Cassegrain, or Gregorian, the same system of adjustment is available. Take out the lens of the eyepiece, leaving only an eye-stop for the purpose of centering the eye in the tube. On placing the eye at this hole an image of the large mirror will be seen reflected in the small mirror, probably not central. By the three screws at back of small mirror adjust it till the rim on cell of large mirror appears to be concentric with the rim of the small. Up to this point of the process the large mirror may, if desired, remain covered; now uncover large mirror and an image of the small mirror will be seen in the centre of the image of the large mirror as reflected on the small. If this be not central make it so by adjustment of the three screws at back of *large* mirror. When making this last adjustment let the telescope b nearly vertical. If a greater degree of accuracy be aimed at tha is attainable by above process it is best to use the collimatin arrangements of Dr. Stoney.

Fig. 4.



Referring to Fig. 4.—The point that is really essential is that the axis of the eyepiece tube (and consequently optical axis of eyepiece) should coincide with the axis *d e* of a central pencil from the great mirror. It is not by any means necessary that the angle *c d e* should be 90°. Dr. Stoney mounts his eye-tube, *not* rigidly to side of tube, but on a plate which is adjustable thereon by three screws, so that he can alter the direction of the axis of the eye-tube.

To test the adjustment the eyepiece is removed and a tube is inserted in its place, carrying an achromatic lens of as large a size as will fit the tube, and at a certain distance behind this lens an eyepiece in focus of which is placed a pair of platinum wires nearly touching, but insulated from, one another, and mounted in an insulated ring. These are connected to a small induction-coil and a spark is caused to pass between these points.

The distance between spark and achromatic lens is so arranged that the rays of light from spark after reflection from small mirror to large are *normal* to surface of mirror, and are therefore returned again with the same vergency, and after passing a second time through lens, form *an image* of the spark in same plane as spark itself, and consequently visible through the eyepiece simultaneously with the spark. If all be in perfect adjustment the image of spark should be found superimposed on spark itself. If adjustment has been made nearly right before, a very little turn to screws which attach the eyepiece to side of tube makes all right.

Dr. Stoney states that the gain in definition by this extra fine adjustment is so great as to induce him to make a fresh adjustment for almost every new object he turns the telescope to, particularly as a few seconds suffice for that operation.

Sir J. Herschel used a small collimator telescope, mounted inside tube and pointing directly at great mirror. This was supplied with illuminated cross-lines which were so placed that when the mirrors were in adjustment the cross-lines were visible in centre of field of eyepiece; any want of adjustment could thus at any moment be detected.

In Cassegrain and Gregorian telescopes the exact position and size of eye-stop is very important; *to adjust this*, screw in eyepiece and uncover mirrors, now take a small hand magnifier, say about 1½-inch focus, and with it examine the eye-stop, an image of the small mirror will be found just at this stop.

1st. If this image is not central with stop it should be made so by loosening the screw which holds eye-stop in position and pushing it about till it is concentric with image.

2nd. The stop should then appear just such a size as to admit of the whole of the small mirror being seen, but no skylight around it.

3rd. To test if stops be at right distance from eye-lens, pass the eye (and magnifying-lens) back and forward so as to view the image at a considerable angle as well as directly. If image be (as it ought to be) in plane of eye-stops, it will appear to remain stationary as regards the stop, no matter what angle it is viewed from. If image appears to move to right when eye is placed to left, and to left when eye is placed to right, the stop is too far in or too near eye-lens and *vice versâ*. In no position, whether the stops be viewed directly or at any angle, should any skylight be visible round image of small mirror. Theoretically the eye-stop should be placed a little inside the focus of images of small mirrors, in fact at image of *large* mirror as seen in small; but except in very large instruments the difference is not appreciable.

Howard Grubb.

Harvard Photometry [*].

To say that the volume recently issued from the Observatory of Harvard College is, in this branch of astronomy, epoch making, is to do no more than justice to a work which must henceforth be regarded as at once the foundation and treasury of scientific stellar photometry. The perseverance and skill of Professor Pickering and his assistants are deserving of the highest praise, the prompt publication of their results being not the least important ground for commendation.

The observations described in this volume were begun in October 1879, and ended in September 1882; they consist of 94,476 separate comparisons of brightness of 4260 stars, embracing all stars visible to the naked eye from the North Pole to declination 30° S.

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[*] Annals of the Astronomical Observatory of Harvard College, Vol. xiv. part 1. Observations with the Meridian Photometer. Cambridge University Press, 1884.

Chapter I. contains a description of the Meridian Photometer designed by Professor Pickering and also the method of observation. This efficient apparatus has many advantages over other forms of photometer. There is no loss of time in identifying stars, and the observations being made at the star's greatest altitude, facilitates the computation of atmospheric absorption; the form of instrument adopted being that of the *broken transit*, the eye is always in the same position relative to the eyepiece, and receives the whole emergent pencil. Instead of using an artificial light for comparison, the Pole star was employed. Two persons are required to work the instrument: one brings into the field the star to be measured and also enters the circle-readings, while the other keeps the two stars near each other in the field, and turns the Nicol to make the images equal. Both stars are seen with the same magnifying-power, and on a background which is the combined effect of the light of those parts of the sky seen through the two object-glasses; they are therefore similarly affected, and errors arising from differences in the brightness of the sky near the two stars, due to moonlight, twilight, or other causes, are eliminated.

In Chapter II. is a summary of the whole series of observations, and a discussion of the variation in brightness of the Pole star. This star being the standard of comparison ought to be of constant brightness; no pains were spared in this investigation, for besides the long series of observations of 100 circumpolar stars, special naked-eye estimates were made of Polaris, with several stars in its neighbourhood and of nearly equal brightness. These observations do not afford any evidence of actual variation of Polaris during the three years. Some interesting results were derived from these observations (page 42), showing that the relative positions of the stars affect the estimate of brightness both in the photometer and by the naked eye. This systematic error was avoided throughout the series by repeating the measures with the images reversed.

Chapter III. is devoted to Atmospheric Absorption. Observations of 100 circumpolar stars between N.P.D. 15° and 30°, at their upper and lower culminations, were made for this investigation. Afterwards stars near the horizon were observed. The deduced coefficient of atmospheric absorption is 0·25 × secant Z.D., which means that stars in our zenith would appear to an observer above our atmosphere one quarter of a magnitude brighter than they do to us, while stars at Z.D. 60° would appear brighter by half a magnitude.

To enable comparisons being made between the photometric determinations of the light of stars and that obtained by direct eye-estimates of relative brightness, a series of observations (Chapter IV.) were undertaken, and the results embodied in the general catalogue, which takes up the whole of Chapter V. This catalogue is most complete and exhaustive. The stars are arranged in order of R.A. for the epoch 1880·0, references at the same time being given to the catalogues of Bayer, Flamsteed, the B.A.C., and the

'Durchmusterung.' Then follow the declination, and the magnitudes as given in Argelander's 'Uranometria Nova,' the 'Atlas Cœlestis' of Heis (many of the errors of which catalogue Prof. Pickering has here corrected), the 'Durchmusterung,' and the 'Uranometria Argentina' of Gould; the results of Chapter IV. complete the left-hand page. On the right-hand page are given the results of the photometric measures, and comparisons of these with the estimates and measures of fourteen standard authorities from Ptolemy down to the present time; the novel method adopted to express residuals is an improvement on the usual custom, and greatly economizes space. The column "Remarks" is presented in a similarly abridged form; and altogether this right-hand page presents an amazing amount of information concerning each star in very small space. There is also a column devoted to the colour of stars, as furnished by Mr. Franks of Leicester. Professer Pickering has chosen for his scale of magnitudes that recommended by Pogson, on which the magnitudes follow according to the strict geometrical ratio, represented by the number whose logarithm is 0·4. Thus a sixth mag. is exactly $\frac{1}{100}$th the brightness of a first magnitude. This catalogue is therefore the first extensive one based on so accurate a scale.

A second volume is promised, which, from an outline of its contents given by Prof. Pickering at the September meeting of the American Association, ought to prove interesting.

T. Lewis.

Second Report of the Committee on Standards of Stellar Magnitudes.

The first report of this Committee ('Observatory,' Vol. iv. No. 55, p. 327) included a plan for the determination of standards for stars fainter than the tenth magnitude. Twenty-four bright equatoreal stars were chosen, and the standards were to be selected from the regions following them from two to six minutes of time and not differing in declination from the leading stars by more than five minutes of arc. The observations described below have been made at the Harvard College Observatory, unless otherwise stated. The light of each of the leading stars has been determined on from seven to eighteen nights with the meridian photometer. Charts have been constructed of all the stars visible with the fifteen-inch telescope, in all but three of the regions from which the standards are to be selected. Most of these charts have been submitted to a careful scrutiny with the fifteen-inch telescope of the Washburn Observatory. An important test of the completeness of the charts is thus afforded.

In the following table three successive columns give the names of the twenty-four leading stars and their approximate right ascensions and declinations for 1880. The next two columns give the number of nights on which they were observed with the meridian photometer, and the resulting magnitude. The details of these measures, and a comparison with various other determinations of their light will be found in the Harv. Observ. Annals, Vol. xiv. The last columns give the number of stars in each of the charts, and the corresponding number of stars contained in the same portions of the 'Durchmusterung.'

Stars suitable for standards must next be selected by the help of the charts. The light of these stars should then be measured in as many different ways as possible. The Committee will be much indebted for aid that may be rendered them in this portion of their work. The early publication of the charts now becomes a matter of importance, as it would permit their immediate use for various purposes.

Name.	R. A. 1880.	Dec. 1880.	No. of Nights.	Phot. Mag.	Stars on Chart.	DM. Stars.
	h m	° ′				
γ Pegasi	0 7·1	+14 31	13	3·04	49	3
θ Ceti..............	1 18·0	− 8 48	12	3·77	27	—
α Piscium	1 55·9	+ 2 11	12	3·99	—	1
α Ceti	2 56·0	+ 3 37	11	2·68	—	2
γ Eridani	3 52·4	−13 51	10	3·05	30	—
α Tauri	4 29·0	+16 16	16	1·00	19	3
ε Orionis	5 30·1	− 1 17	16	1·76	42	6
γ Geminorum ...	6 30·8	+16 30	17	2·00	150	5
α Canis Minoris...	7 33·0	+ 5 32	15	0·46	96	3
ε Hydræ...........	8 40·4	+ 6 51	7	3·58	64	4
α Leonis	10 2·0	+12 33	15	1·42	39	1
θ Leonis	11 7·9	+16 5	10	3·47	24	3
η Virginis	12 13·8	+ 0 0	10	4·05	23	2
α Virginis	13 18·9	−10 32	13	1·23	30	—
α Boötis	14 10·2	+19 48	13	0·03	25	1
β Libræ	15 10·5	− 8 56	14	2·74	39	—
δ Ophiuchi	16 8·1	− 3 23	11	2·77	48	—
η Ophiuchi	17 3·5	−15 34	10	2·62	100	—
η Serpentis	18 15·1	− 2 56	10	3·35	7	—
δ Aquilæ	19 19·4	+ 2 53	14	3·46	—	3
θ Aquilæ	20 5·1	− 1 11	10	3·39	110	2
β Aquarii	21 25·2	− 6 6	10	3·14	52	—
α Aquarii	21 59·6	− 0 54	10	3·16	48	0
α Pegasi	22 58·8	+14 34	18	2·61	29	3

Respectfully submitted,

EDWARD C. PICKERING, *Chairman*; LEWIS BOSS, S. W. BURNHAM, ASAPH HALL, WILLIAM HARKNESS, EDWARD S. HOLDEN, SIMON NEWCOMB, C. H. F. PETERS, ORMOND STONE, C. A. YOUNG.

The Chromosphere in 1884.

An automatic spectroscope by Browning is employed for these daily measures of the chromosphere, and during the year 1884 the same dispersion, viz. 6 prisms of 60°, has been invariably used. The greater part of the year has been very favourable for these observations, and the increase in the results is well marked in the first column of figures. The mean height of the chromosphere, which differed little in 1882 and 1883, and attained its maximum in May 1883, has fallen away rapidly this year. This is partly due to the continuance of very low readings throughout August and September, but it remained to the end of the year considerably below the average of the three preceding years. There is also a great diminution in the numbers of the prominences and some falling off in their average height. The low reading 6″·41 for the chromosphere on June 30 was the more remarkable, as the prominences were numerous, and the highest recorded for the month was observed that day.

The number of observed displacements of the C line differed but little in the last two years, but the amount of displacement was slight in 1884 compared with that seen in 1883. The distortions recorded during the months of April and May 1882 were far greater than any observed during the last two years. The line K 654·3 was seen on five different days as a bright line in the chromosphere, C being on each occasion very bright, but short. Both lines were about the same height, but their intensities were as three to ten.

	No. of days of obs. per month.	Mean height of chromosphere excluding prominences.	Mean height of prominences.	Mean extent of prominence arc.	Highest prominences.	No. of days on which lines were observed distorted.
		″	″	° ′ ″	′ ″	
Jan.	1	8·11	21·35	11 13 0	0 40·99	0
Feb.	5	8·37	27·16	44 53 24	1 1·92	3
March ...	2	7·69	23·91	29 21 0	1 21·13	1
April	7	8·36	28·67	43 31 51	1 34·79	2
May	13	8·44	27·52	48 10 46	1 35·22	4
June	14	8·36	23·61	36 48 21	1 19·00	2
July	4	8·11	22·74	31 14 15	0 52·52	0
Aug.	13	7·10	25·42	34 6 42	1 25·40	4
Sept.	9	7·26	23·87	20 53 40	0 59·78	2
Oct.........	6	7·83	29·89	17 26 10	1 3·20	1
Nov.	7	7·93	28·78	14 31 26	2 2·12	2
Dec.	7	7·75	25·92	17 1 26	0 55·51	3
880	5·3	7·93	23·46	23 21 23	1 7·40	...
881	4·3	8·04	24·61	33 18 26	1 11·93	...
882	5·8	8·24	24·55	40 56 47	1 14·10	2·8
883	6·1	8·27	27·23	41 24 15	1 35·43	1·9
884	7·3	7·94	25·74	29 6 0	1 14·30	2·0

Stonyhurst Observatory,
Jan. 16, 1885.

S. J. Perry.

The Pons-Brooks Comet Meteors.

Anticipating a possible return this year of the meteor-stream emanating, evidently, from the Pons Comet, and which feebly manifested itself last year, when the Earth crossed the plane of the comet's orbit, short watches were taken on the evenings of Dec. 3, 5, 7, 8, and 9 (the 4th and 6th being overcast). Although some 10 meteors were recorded during the 4 hours' watching, only one was observed which could be attributed to the stream in question, and this one was observed on the 8th at $6^h\ 25^m$ C.M.T.

The meteor was a bright one = 1st mag., and started near ζ Ursæ Minoris, at R.A. 235° +79½°, and ended at R.A. 270° +81°. It left a presistent streak and shot very rapidly. While looking for the Pons meteors, a feeble shower of quite bright, short, slow-moving, and orange-coloured meteors manifested itself on the 3rd and 5th, with a radiant at about R.A. 250° +66°, 4 meteors.

The only radiant-point I can find in the catalogues accessible to me, as in any way confirmatory of this position, is one deduced by Mr. W. F. Denning from the shooting-stars observed by the members of the Italian Meteoric Assoc. in 1872, from Nov. 25 to Dec. 31, at R.A. 245° +65°, 4 meteors.

The following meteor-tracks were mapped :—

No.	Date 1884.	Mean Time.	Magnitude.	Observed path.				Weight.	Remarks.
				From		To			
				R.A.	Dec.	R.A.	Dec.		
		h m		°	°	°	°		
1.	Dec. 3.	5 45	2	280	+73	297	+74	3	Short and slow.
2.	,, 5.	5 30	4	214	+72	189	+71½	2	Short and slow.
3.	,, 5.	5 35	2	320	+61·5	330	+55½	3	Short and rather slow.
4.	,, 5.	5 42	2	220	+75	204	+77	4	Short and slow.
5.	,, 8.	6 50	5	262½	+74	241	+80	3	Short and rapid.
6.	,, 8.	6 52	5	280	+74	270	+75	3	Very short and rapid.
7.	,, 8.	7 10	1	275	+71½	261	+52½	4	Long and very slow; 3 sec.; ended near β Draconis.
8.	,, 9.	7 45	>1	54	+ 6½	30	−12	3	Very long and rather slow; 2 sec.; blue

Centre of observation, Ursæ Minor.

Errata.—In my article ('Observatory,' Feb. 1884) two mistakes occur, as follows :—For Dec. 51°·5 read 58°·5, and Dec. 62° should be 52°·0. The latter mistake is my own, and also occurs in the Astr. Nachr. No. 2592.

Cambridgeport,
Dec. 15, 1884.

Edwin F. Sawyer.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Old Occultations of Aldebaran.

SIR,—

The coming occultations of Aldebaran will be watched with somewhat different instruments from that which is recorded by Street in his 'Astronomia Carolina' as having been observed in the year 509—probably the earliest recorded observation of the kind, unless some such are to be found in the Chinese annals. The following is the account in Street's own words:—

"Anno Christi 509 March 11th, after the going down of twilight, the moon was seen following the clear star of the Hyades at most half a digit, but she seemed to hide him. The star was applyed to the part by which the illuminate limb of the moon was bisected. Then was the moon according to true motion in ♉ 16° 30′." He goes on to mention that Bullialdus found the observation in an ancient Greek manuscript, and that though the latter supposed it to have been made at Athens, it was probably in some other meridian at least two hours more to the east. Doubtless some such observations are recorded in the middle ages; and probably Aldebaran is the star referred to by Pingre in his 'Cometographie,' tom. i., when he says in A.D. 547 "une étoile entra sur la disque de la lune;" but there seems a doubt about the exact date.

In March 1497 Copernicus observed an occultation of the star at the fifth hour of the night.

On March 4, 1607, Fabricius observed an apparent projection of Aldebaran on the Moon's disk with the naked eye.

The singular point seems to be, not that projection is noticed on the Moon's limb in the case of a red star, but that some observers should see the occultation without any projection at all. In the 'Monthly Notices' for November 1829 the impressions of observers of the occultations of August 21 and October 15, in England and on the continent, are given. Nearly every observer in this country noticed the star embedded on the body of the Moon for 2 or 3 seconds. On October 15 a few observers, especially those at Paris, noticed no projection. As an exception to this, one observer, M. Francœur, at Paris, says:—"The star was seen projected on the disk about 4 seconds. As the star approached the Moon, its red tint became diminished till it resembled the colour of the planet Venus. Instrument achromatic."

Melplash Vicarage, Bridport,
Jan. 13, 1885.

Faithfully yours,
S. J. JOHNSON.

NOTES.

THE WASHINGTON OBSERVATORY.—The Superintendent of the U. S. Naval Observatory, Commodore S. R. Franklin, has issued his Report for the year ending Oct. 30, 1884. Some changes in the internal arrangements of the Observatory are announced, the most important of which is the organization of a Board consisting of the Superintendent, the Senior Professor of Mathematics, and the Senior Line Officer, whose duty it is to meet from time to time and deliberate upon the conduct of the Observatory. Commodore Franklin also proposes to print and distribute the programme of work for each year as soon as it is completed, in order that other observatories may be fully informed of what is being done at Washington, and thus prevent unnecessary duplication of work. It is also suggested that a Board of Visitors should be appointed to meet annually, and to examine into the conduct of the Observatory, and to make such suggestions as might add to its general usefulness.

The work with the principal instruments appears to be going on in its usual satisfactory way. The 26-inch Equatoreal, in charge of Prof. A. Hall, has been employed for observations of the satellites of Neptune, Uranus, Saturn, and Mars, and for observations of double stars. A few observations have been made for stellar parallax. For several years past the dome has been moved with difficulty. To obviate this, a gas-engine of 4-horse power has recently been successfully connected with the turning machinery.

The Transit-circle is in its usual efficient state. Under Prof. J. R. Eastman's direction, a total of 5420 observations were made during the year, including 98 of the Sun, 84 of the Moon, 151 of the major planets, 45 of the minor planets, and 11 of comets. The other astronomical instruments are also kept in constant use. The Time-Signal Service of the Observatory appears to be in a very satisfactory state. Upon the adoption of what is known as "standard time," the transmitting clock of the Observatory was set to the seventy-fifth meridian time (8^{m} $12^{s}\cdot09$ earlier than Washington time), and the facilities for transmitting time-signals were very much extended, signals being now transmitted from the Observatory over eleven wires.

An Appendix contains a report by Prof. W. Harkness on the state of the reductions of the transit of Venus, Dec. 6, 1882 observations. The measures of the negatives of the transit obtained at the various stations have been completed, the number of photographic plates affording satisfactory results being 932 for the northern hemisphere, and 639 for the southern hemisphere. Very considerable progress has been made in the computations necessary for the reduction of the measurements of these negatives. The ordinary astronomical observations for time and latitude have also been completely reduced for some of the stations, and some progress has been made in the reduction of those made at other stations. We hope before long to be able to congratulate Prof. Harkness on the successful termination of his arduous labours.

AMOUNT OF THE ATMOSPHERIC ABSORPTION *.—In an important paper on this subject, Prof. Langley has arrived at results which are startlingly at variance with what has been hitherto supposed to be the amount of the absorption of solar and stellar light and heat by our atmosphere. All previous investigators agree in assigning an absorption of about 20 per cent. of the whole (at sea-level), whilst Prof. Langley has been led to think that the true amount is more than double of this quantity; and he has deduced this result, not from theoretical considerations alone, but also from experiments made both near the sea-level and at great altitudes. Prof Langley points out that most persons who have occupied themselves with researches on this subject have, in the reduction of their observations, made use of the exponential formula of Pouillet, viz. Ap^{ϵ}, to express the amount of light or heat which reaches the observer after transmission through ϵ strata—the fundamental (and erroneous) assumption being that p, the coefficient of transmission, is constant. But the solar energy is the sum of an infinite number of radiations, and these may be conceivably influenced in an infinite diversity of ways by the different atmospheric constituents. Thus the effect of the action of the grosser dust particles is to produce a general absorption of all rays, whilst the effect of the molecular absorption is of a selective character, so that after absorption what remains is not only less in amount, but altered in kind. By working on these lines Prof. Langley arrives at the result that the coefficient of transmission is never a constant, and (as determined from the customary formula) is always too large, and always larger and larger as we approach the horizon; and that the original light or heat of the heavenly body as found by the photometric and actinometric processes, and the formulæ in almost universal use, is always too small. By a numerical example, the numbers employed in which do not differ much from the results of actual observation, and the assumption being made that the total radiant energy in the Sun or star is divided into a certain finite number of parts, each one of which has its own rate of absorption, Prof. Langley shows that the successive values of the absorption found by comparing a zenith observation with three successively lower altitudes are (by the ordinary method) 21 per cent., 19 per cent., and 18 per cent., whilst the true value is 41 per cent. It thus appears that the actual error of results deduced by the ordinary method may well be enormously larger than the mere discordance of separate observations would lead us to suppose; and although this may be a matter of small importance were the stars alone in question, with the Sun it is otherwise, as his absolute light and heat are of paramount importance in problems of solar physics or of meteorology. Prof. Langley will be doing good service to our science by continuing his researches on this subject, for the observational part of which his bolometer seems peculiarly suitable.

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* American Journal of Science, Vol. xxviii. p. 163.

THE DISTRIBUTION OF THE STARS OF THE 'BONNER DURCHMUSTERUNG' *.—Some fifteen years ago Littrow published a paper on the distribution in declination of the stars in the northern hemisphere which are contained in Argelander's 'Durchmusterung,' the stars being arranged according to magnitude †. Unfortunately in his work Littrow paid no attention to order of R.A.; and it is to supply this deficiency that Prof. Seeliger has taken up the subject, and has deduced some interesting results from his discussion of it. The work has had to be done entirely *de novo*, as it was found that no use could be made of Littrow's investigation except as a check on the numerical accuracy of the fresh enumeration. Prof. Seeliger did not wish to confine himself to telescopic stars only; but as the naked-eye stars have been frequently dealt with in connection with the question of distribution, especially by M. Houzeau in his 'Uranométrie Générale,' he decided to group these stars by themselves, and to proceed by half-magnitudes for the fainter stars. Thus Class I. contains the stars of magnitudes 1 to 6·5 inclusive, Class II. magnitudes 6·6 to 7·0, and so on to Class VII., which contains stars of magnitudes 9·1 to 9·5. Prof. Seeliger then gives the number of stars in each class arranged in zones embracing 5° in declination, and grouped by intervals of 40^m in R.A. The totals for each class are:—Class I. 4120; Class II. 3887; Class III. 6054; Class IV. 11168; Class V. 22898; Class VI. 52852; and Class VII. 213973. The total number of stars thus counted is 314,952, and adding to this 126 objects which are set down in the DM. as nebulæ or variables, there results the grand total 315,078. A summation of all the numbers of the DM., as given in vols. iii.–vi. of the Bonn Observations, gives for this total 315,089, so that Prof. Seeliger's numeration is sensibly correct; but there appear to be some considerable errors in Littrow's counts. In order to show the influence of the Milky Way on the distribution of the stars in the N. hemisphere, Prof. Seeliger has arranged them in eight zones, each being 40° of Galactic polar distance in width (the pole of the Milky Way being situated in $A = 12^h\ 49^m$, $D = +27°\ 30'$); and from a discussion of the numbers of stars thus obtained he arrives at what he denominates the "gradient" for each class of stars, this being a quantity which, for the mean of the zones, indicates the defect in the number of stars of each class from the number which occurs in the Milky Way itself. The gradient for Class I. is decidedly smaller than for the other classes, and that for Class VII. slightly larger; that is, the brighter stars (of mags. 6 to 6·5 inclusive) are more equally scattered over the heavens than those of the other classes, and the faintest class here discussed shows a tendency to increase in numbers as we approach the Milky Way rather more decidedly than the stars of Classes II. to VI. But the difference of gradient for Classes II. to VII. is very small.

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* Sitzungsbericht der mathem.-phys. Classe der k. bayer. Akad. d. Wiss. 1884, Heft 4.

† Astronomische Nachrichten, No. 1741.

Treating in the same manner the numbers given by Houzeau at p. 52 of the work referred to above, Prof. Seeliger finds the gradients for stars of magnitudes 1 to 3 inclusive, and of 4 to 6 inclusive. It appears that the latter are much more equally scattered than the former (as Houzeau also remarks), though the stars of magnitudes 1 to 3 are more equally scattered than stars fainter than mag. 6·5. From the constancy of the gradients for stars from mag. 6·6 to 9·5, Prof. Seeliger draws the conclusion that we should consider the star-system to which our Sun belongs not as a flat disk, but as more or less spherical in form, but such that the stars in the neighbourhood of a certain plane (*i. e.* that of the Milky Way) lie closer together than in other parts. It may be remarked, however, that it appears from Prof. Holden's investigations ('Observatory,' Vol. vii. p. 250) that there is no such approach to uniformity of distribution when we come to deal with stars fainter than the 11th magnitude. Prof. Seeliger also makes some calculations with the view of determining the centres of gravity of the stars in each of the above-mentioned classes; but these are avowedly of a partial character only, and possess little interest until the investigation can be extended over the whole sphere of stars. We hope that Prof. Seeliger will continue his researches in this direction, making use of Dr. Gould's extension of Argelander's 'Durchmusterung' to within 10° of the South Pole.

A DETERMINATION OF A FUNDAMENTAL MERIDIAN FOR AUSTRALIA [*].—In this paper Prof. Auwers has given an elaborate investigation of the longitudes of Windsor, N.S.W., and of Melbourne from observations of occultations of stars by the Moon made during the years 1873–76 inclusive. The Australian observations have been compared with those made at the principal European observatories and at Washington. The mean results are—for Windsor $10^{h}\ 3^{m}\ 21^{s}{\cdot}25$, and for Melbourne $9^{h}\ 39^{m}\ 53^{s}{\cdot}12$, both east of Greenwich. Prof. Auwers takes the meridian of Sydney as the fundamental meridian for Australia, and from the differences of longitude between this observatory and those of Windsor and Melbourne respectively, which have been determined telegraphically, he finds for the longitude of Sydney, $10^{h}\ 4^{m}\ 49^{s}{\cdot}82$ East. Determinations of longitude by the method of Moon culminations have also been made for Sydney and Melbourne—by Scott and Russell for the former, and by Ellery for the latter; and the values of the longitude of Sydney resulting from these determinations are $10^{h}\ 4^{m}\ 48^{s}{\cdot}24$ and $10^{h}\ 4^{m}\ 50^{s}{\cdot}11$ respectively, and therefore the most probable value resulting from Moon culminations is $10^{h}\ 4^{m}\ 49^{s}{\cdot}17$. Combining this result with that obtained from occultations, giving double weight to the latter, the final longitude of Sydney, determined by absolute methods, is

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[*] Astronomische Nachrichten, No. 2635–36.

$10^h\ 4^m\ 49^s{\cdot}60$ East. The corresponding values for the other Australian Observatories are :—

Windsor = Sydney − $1^m\ 28^s{\cdot}83 = 10^h\ 3^m\ 20^s{\cdot}77$ E. of Greenwich.
Melbourne = Sydney − 24 55·43 = 9 39 54·17 " "
Adelaide = Melb^{ne} − 25 33·75 = 9 14 20·42 " "

These values agree closely with those which have been found by connecting the Australian stations telegraphically with Singapore and thus with Madras, the results of which are given in the 'Monthly Notices,' Vol. xliv. p. 183.

PUBLICATIONS OF THE WASHBURN OBSERVATORY.—Prof. Holden has recently published the second volume of the Publications of the Observatory under his charge, referring chiefly to the work done during 1883. The *pièce de resistance* of the volume is the portion which treats of the statistics of stellar distribution, the substance of which has been already published in 'The Observatory' by Prof. Holden (Vol. vii. p. 249 *et seq.*), and need not therefore be further alluded to here. Other sections of the volume contain—a description of the meridian circle, with examination in detail of its various parts, as they affect the instrumental errors; results of some of the observations which have been made with this instrument; lists of new double stars and nebulæ, and of red or coloured stars; occultations of stars by the Moon; two drawings of Saturn; tables of precession in R.A. and declination; investigation of the micrometer of the 15-inch equatoreal; a new mode of observing with the prime-vertical transit; and some meteorological observations. The meridian circle was constructed by the Repsolds of Hamburg, the object-glass (made by Alvan Clark and Sons) having a clear aperture of 4·8 inches and a focus of 57·6 inches. The instrument is furnished with two divided circles each read by four microscopes. It is described by Prof. Holden as being "all but perfect," and especially fitted for doing fundamental work; and we venture to express a hope that the instrument will be devoted to work of this class, after the model of the Leiden observations. On account of the position of the Observatory, near the southern shore of Lake Mendola, stars culminating north of the zenith must be observed through air which lies over the lake, while stars south of the zenith are observed through air which lies over land. The refraction for low circumpolars and for extreme south stars may be different, except during the winter months when the lake is frozen and covered with snow, so that the conditions north and south are then similar. In summer Prof. Holden thinks it may be necessary to determine two different coefficients of refraction. Provided, however, that the nadir can be continuously observed, it would seem that by suitably choosing the seasons of observation, fundamental work is practicable. But observations of the Sun would require very careful treatment, as the summer and winter conditions are so different. Investigations of the flexure, of the corrections to the Pulkowa refractions to fit them to the conditions of the Washburn observations, and of the latitude are in progress. Astronomers will await with interest the results of Prof. Holden's investigations on refraction, as a difference in the refractions north and south of the zenith was at one time supposed to exist at Melbourne, though further researches on the subject (see 'Monthly Notices,' Vol. xli. p. 346) would appear to indicate that such is not the case.

SOCIETY FOR POPULAR ASTRONOMY AT BASLE.—Thanks to the efforts of Herr Rudin Hefti, the town of Basle possesses a society having for its object the popular study of Astronomy. It has been in existence since May 1883 and has enjoyed deserved success. At the ordinary meetings of the Society, the President, Herr Hefti, or some other well-qualified person, lectures on the real and apparent motions of the planets and on other important astronomical subjects. A telescope of 5 inches aperture, properly equipped, has been placed at the disposal of the members, who can thus obtain occasional views of the heavenly bodies. The lectures are well attended. A reproduction of Foucault's pendulum experiment was given at the opening meeting, before 600 people, in one of the churches at Basle. Similar societies might, we venture to think, be set on foot, with good results, in not a few English towns.

BREWSTER'S LINE Y.—Prof. Piazzi Smyth, in a paper read before the Royal Society of Edinburgh on Dec. 17, 1883, and recently published, gives a short but interesting history of the dark line in the extreme red, which Brewster and Gladstone distinguish by the letter Y. He points out that Captain Abney, in the Philos. Trans. for 1880, has given the letter Z to this line—the true Z line, being a telluric line, was not visible in the spectrum of a high sun on which Captain Abney was working. He further notices M. Becquerel's important discovery (Comptes Rendus, July 1883) that the Y line was due to sodium vapour. Prof. Smyth was not himself able distinctly to produce the Y line from sodium, but in his search discovered three new air-lines further towards the dark red than any recorded in Watts's Index. Of these three lines, the centre one is triple, and Prof. Smyth has proved it to belong to nitrogen, whilst the one furthest in the red is an oxygen line. A series of sketches of the district x to a is given in the plate which accompanies the paper.

ENCKE'S COMET.—Herr Tempel writes "that the discovery of Encke's Comet on Dec. 13, 1884, was not merely 'supposed,' is attested by the following observations, which are better than those of Dec. 13, because the comet has become brighter:—

1885.		App. R.A.	App. Dec.
Jan. 3..	6^{h} 42^{m} 33^{s} Arcetri M.T.	22^{h} 57^{m} $8^{s}.47$	$+4^{\circ}$ $3'$ $39''.4$
8..	6 17 6	23 1 57.33	+4 20 32.1
8..	6 47 42	23 1 58.62	4 20 41.4

"The last observation was obtained with the Boguslawski differential micrometer, and is better than the preceding ring-micrometer comparisons. The comet appears bright with the Amici prism and shows a granular centre, and was also seen with the finder as a small faint nebula. Unfortunately we had cloudy weather almost the whole of December, and since that time there have been only three fine bright nights in January."

Mr. Barnard, at Nashville, Tennessee, satisfactorily observed the comet on Jan. 2, 1885, at $7^h\ 45^m$ Washington time. Its place was then R.A. $22^h\ 56^m\ 34^s$, Dec. $+4°\ 1'\ 56''$. It has also been observed by Prof. Young at the observatory of Algiers. Its place was close to that given in Dr. Backlund's ephemeris.

FAINT STARS FOR STANDARDS OF STELLAR MAGNITUDE.—Prof. Pickering, Chairman of the Committee on Standards of Stellar Magnitudes, has published and distributed a circular under this heading, accompanied by charts of four out of the twenty-four regions from which it is proposed to select certain stars for standards of the magnitude of faint stars. The regions represented in these four charts are those from 2^m to 6^m, following the bright stars γ Pegasi, ϵ Orionis, η Virginis, and η Serpentis. Each region extends 5′ north and 5′ south of the declination of the corresponding bright star. "It is desirable," Prof. Pickering writes, "that these charts should be made as complete as possible, and it is hoped that astronomers having the use of powerful telescopes will assist in accomplishing this object. They will confer a favour upon the Observatory of Harvard College by comparing these charts with the regions which they represent, and marking upon them the places of any additional stars which may be visible. Some indications of the comparative brightness of these stars would also be desirable." Astronomers who may be disposed to take part in this work are requested to send the chart of any particular region, as soon as possible after its revision, to the Harvard College Observatory.

A NEW VARIABLE.—Dr. Dunér has discovered a new variable star in Ophiuchus, R.A. $16^h\ 19^m\ 46^s$, Dec. 12° 8′·5 S. According to Dr. Dunér, it varies from the 7th to the 10th mag., and has a period of 311 days. Its next maximum will be about the beginning of April 1885.

DR. GILL has obtained a sum of money from the Government Grant for Scientific Purposes in order to enable him to set on foot a photographic survey of the Southern heavens. Mr. C. Ray Woods is proceeding to the Cape for the purpose of taking the requisite photographs. Mr. Woods intends to continue the work of photographing the Corona, which he lately undertook, under Dr. Huggins's direction, in Switzerland.

THE supposed new minor planet No. 245 has been found to be identical with No. 208 (Lacrimosa).

THE minor planets Hersilia (206) and Isabella (210), which had not been observed since the year of their discovery (1879), have recently been re-observed.

HERR PALISA, being desirous to raise funds for his intended expedition to observe the total solar eclipse of August 1886, will sell the right of naming the minor planet No. 244 for £50.

THE number of chronometers on trial at the Royal Observatory, Greenwich, for purchase by the Admiralty, is 45.

WE would draw attention to the occultation of Aldebaran on the afternoon of Feb. 22.

Approximate Times of Transit of the great Red Spot and Equatoreal White Spot across the Central Meridian of Jupiter in February 1885.

Day of Month.	Red Spot on C.M.	White Spot on C.M.	Day of Month.	Red Spot on C.M.	White Spot on C.M.
	h	h		h	h
1.	13·1	8·3	15.	14·5	16·3
2.	9·0	13·8	16.	10·3	11·9
3.	14·7	9·4	17.	16·1	7·6
4.	10·5	14·9	18.	12·0	13·1
5.	16·2	10·6	19.	7·9	8·7
6.	12·1	16·1	20.	13·6	14·2
7.	17·9	11·7	21.	9·5	9·9
8.	13·8	7·4	22.	15·2	15·4
9.	9·6	12·9	23.	11·1	11·0
10.	15·4	8·5	24.	7·0	6·7
11.	11·2	14·0	25.	12·8	12·2
12.	17·0	9·7	26.	8·7	7·8
13.	12·9	15·1	27.	14·3	13·3
14.	8·8	10·8	28.	10·2	9·0

The red spot was observed here as it passed the central meridian on Jan. 12, $16^h\,31^m$ (Long. $319^{\circ}{\cdot}5$), and on Jan. 13, $12^h\,20^m$ (Long. $324^{\circ}{\cdot}9$). The rotation period for this object appears to be shorter during the present opposition than during the previous two years, during which the rate remained constant at $9^h\,55^m\,39^s{\cdot}1$ ($=870^{\circ}{\cdot}307$ daily).

The equatoreal white spot preceded Mr. Marth's First Meridian by about 68 minutes on Jan. 12 and 13. Its greatly accelerated motion, referred to in the last number of the 'Observatory,' has become somewhat moderated. Between Dec. 20, 1884, and Jan. 13, 1885, the rotation-period was $9^h\,49^m\,47^s{\cdot}3$, so that the spot became central a few minutes later than the time given in our list for January. It is, however, utterly impossible to predict the

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS (East).

		h
Feb.	8	14·5
	9	13·1
	10	11·7
	11	10·3
	12	8·9
	13	7·5
	14	6·2
	26	12·3
	27	10·9

MIMAS (West).

		h
Feb.	1	13·0
	2	11·6
	3	10·2
	4	8·8
	5	7·4
	6	6·0
	7	4·6
	18	12·1
	19	10·7
	20	9·3
	21	7·9

ENCELADUS.

		h
Feb.	1	18·5
	3	3·4
	4	12·3
	5	21·2
	7	6·0
	8	14·9
	9	23·8
	11	8·7
	12	17·6
	14	2·5
	15	11·4
	16	20·3
	18	5·1
	19	14·0
	20	22·9
	22	7·8
	23	16·7
	25	1·6
	26	10·5
	27	19·4

TETHYS.

		h
Feb.	1	15·1
	3	12·4
	5	9·7
	7	7·0
	9	4·3
	11	1·6
	12	22·9
	14	20·2
	16	17·5
	18	14·8
	20	12·1
	22	9·4
	24	6·7
	26	4·0
	28	1·3

TITAN.

Feb.	4	20·6
	20	18·8

DIONE.

		h
Feb.	3	4·1
	5	21·8
	8	15·4
	11	9·1
	14	2·8
	16	20·5
	19	14·1
	22	7·8
	25	1·6
	27	19·3

RHEA.

Feb.	4	7·2
	8	19·5
	13	7·9
	17	20·3
	22	8·8
	26	21·2

HYPERION.

Feb. 20 18·1

Apparent Elements of the Ring.

Greenwich Noon.	Position-angle of Minor Axis.	Latitude above plane of Ring. Earth.	Latitude above plane of Ring. Sun.
Feb. 7	354° 58′	26° 51′ S.	26° 49′ S.
27	354 58	26 55 S.	26 48 S.

Greenwich Noon.	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Semi-maj. axis of Orb. of Titan.
Feb. 7	44″·02	19″·88	29″·28	13″·22	198″·1
27	42 ·46	19 ·22	28 ·24	12 ·78	191 ·0

The semi-major axis of the orbit of Titan is given as well as the position-angle of the minor axis of the ring, in order to furnish means for roughly inferring the distances and position-angles of the satellites from the diagram given in Plate I., opposite p. 34.

Astronomical Memoranda, February 1885.

Equation of Time:—Sun *after* Clock, Feb. 1, $13^m\ 53^s$; Feb. 11, $14^m\ 28^s$; Feb. 21, $13^m\ 47^s$; Feb. 28, $12^m\ 40^s$.

Sidereal Time at Mean Noon:—Feb. 1, $20^h\ 47^m\ 26^s$; Feb. 11, $21^h\ 26^m\ 51^s$; Feb. 21, $22^h\ 6^m\ 17^s$; Feb. 28, $22^h\ 33^m\ 53^s$.

Sun.	Rises.		Sets.		Position-angle of axis.		Heliogr. co-ordinates of centre of disk.			
							Lat.		Long.	
	h	m	h	m	°	′	°	′	°	′
Feb. 5....	19	33	4	55	345	44	−6	27	236	21
10....	19	23	5	4	343	52	−6	43	170	30
15....	19	14	5	13	342	7	−6	56	104	41
20....	19	4	5	22	340	32	−7	5	38	49
25....	18	54	5	30	339	5	−7	12	332	59

The Moon.

rises.			*rises.*			*sets.*			*sets.*		
	h	m		h	m		h	m		h	m
Feb. 1	7	38	Feb. 8	15	16	Feb. 15	6	0	Feb. 22	14	9
2	8	51	9	16	6	16	7	7	23	15	12
3	10	3	10	16	52	17	8	17	24	16	7
4	11	12	11	17	32	18	9	27	25	16	54
5	12	18	12	18	8	19	10	38	26	17	34
6	13	21	13	*sets.*		20	11	49	27	18	8
7	14	20	14	4	54	21	13	1	28	18	38

Feb. 6^{d} 10^{h} 38^{m} Last Quarter. Feb. 14^{d} 14^{h} 22^{m} New Moon.
21 22 31 First Quarter. 28 16 0 Full Moon.

Planets for February 1885.

Day.	Planet.	R.A.			Dec.			Diam.	Rises.		Tr.		Sets.	
		h	m	s	°	′		″	h	m	h	m	h	m
1	Mercury..	19	20	50	22	3	S.	6·0	18	33	22	35	2	37
11		20	18	35	20	51	S.	5·6	18	44	22	54	3	4
21		21	21	36	17	31	S.	5·2	18	47	23	18	3	49
28		22	7	25	13	52	S.	5·0	18	44	23	36	4	28
1	Venus ..	19	26	12	22	2	S.	11·0	18	38	22	40	2	42
11		20	19	2	20	7	S.	10·6	18	39	22	53	3	7
21		21	10	13	17	13	S.	10·4	18	32	23	5	3	38
28		21	44	56	14	42	S.	10·3	18	25	23	12	3	59
1	Jupiter ..	10	22	7	11	30	N.	41·4	6	29	13	32	20	35
11		10	17	27	11	58	N.	41·8	5	42	12	48	19	54
21		10	12	28	12	27	N.	42·0	4	56	12	4	19	12
28		10	9	0	12	46	N.	41·8	4	23	11	33	18	43
1	Saturn ..	5	6	14	21	32	N.	17·6	0	14	8	17	16	20
28		5	5	47	21	38	N.	17·0	22	27	6	31	14	35
1	Uranus ..	12	10	54	0	21	S.	4·0	9	19	15	21	21	23
28		12	7	45	0	0			7	28	13	32	19	36
1	Neptune .	3	14	18	16	12	N.		22	57	6	26	13	55
28		3	15	15	16	18	N.		21	11	4	41	12	11

Mars—In conjunction with the Sun Feb. 11^{d} 0^{h}.
Jupiter—In opposition to the Sun Feb. 18^{d} 20^{h}.
Saturn—Stationary Feb. 16^{d} 13^{h}.

Phenomena of Jupiter's Satellites, February 1885.

d	h	m	
1	14	33	i. Ec. D.
	17	15	i. Oc. R.
	18	6	ii. Tr. I.
2	10	8	iv. Ec. D.
	12	12	i. Tr. I.
	14	31	i. Tr. E.
	18	25	iv. Oc. R.
3	9	1	i. Ec. D.
	11	41	i. Oc. R.
	12	9	ii. Ec. D.
	15	49	ii. Oc. R.
4	8	57	i. Tr. E.
5	7	13	ii. Tr. I.
	10	8	ii. Tr. E.
	14	37	iii. Tr. I.
	18	13	iii. Tr. E.
8	16	27	i. Ec. D.
9	7	53	iii. Oc. R.
	13	56	i. Tr. i.
	16	16	i. Tr. E.
10	10	55	i. Ec. D.
	13	25	i. Oc. R.
10	14	45	ii. Ec. D.
	18	5	ii. Oc. R.
11	8	22	i. Tr. I.
	10	41	i. Tr. E.
12	7	51	i. Oc. R.
	9	26	ii. Tr. I.
	12	21	ii. Tr. E.
	17	54	iii. Tr. I.
14	7	12	ii. Oc. R.
15	18	20	i. Ec. D.
16	7	21	iii. Ec. D.
	11	9	iii. Oc. R.
	15	40	i. Tr. I.
	18	0	i. Tr. E.
17	12	49	i. Ec. D.
	15	8	i. Oc. R.
	17	22	ii. Ec. D.
18	10	6	i. Tr. I.
	12	25	i. Tr. E.
19	7	15	i. Oc. D.
	8	41	iv. Ec. R.
	9	34	i. Oc. R.
19	11	38	ii. Tr. I.
	14	33	ii. Tr. E.
20	6	51	i. Tr. E.
21	6	32	ii. Oc. D.
	9	33	ii. Ec. R.
23	10	49	iii. Oc. D.
	14	49	iii. Ec. R.
	17	24	i. Tr. I.
24	14	32	i. Oc. D.
	16	57	i. Ec. R.
25	11	50	i. Tr. I.
	14	9	i. Tr. E.
26	8	58	i. Oc. D.
	11	26	i. Ec. R.
	13	51	ii. Tr. I.
	16	47	ii. Tr. E.
27	8	35	i. Tr. E.
	9	38	iv. Tr. I.
	14	11	iv. Tr. E.
28	8	48	ii. Oc. D.
	12	10	ii. Ec. R.

Occultations, February 1885.

Day.	Star.	Mag.	Disap.	Angle.	Reap.	Angle.
			h m	°	h m	°
1	*d* Leonis........	5	8 1	69	8 57	256
1	B.A.C. 3836	6	15 29	61	16 41	250
1	75 Leonis	5½	17 26	85	18 28	231
1	76 Leonis	6	18 29	45	19 25	274
4	B.A.C. 4591	6	15 13	109	16 11	204
20	38 Arietis	5	7 41	177	8 1	211
22	Aldebaran	1	5 17	36	5 50	344
23	130 Tauri	6	11 3	106	12 2	247
26	B.A.C. 2872	6	3 59	19	4 24	326
26	α Cancri........	4	16 1	64	16 53	264
27	B.A.C. 3407	6	17 24	148	17 37	178
28	35 Sextantis	6	11 59	39	13 4	274

The R.A. and N.P.D. of Planets and the Solar Co-ordinates are given for Greenwich Mean Noon; in all cases Greenwich Mean Time is used, reckoning from noon to noon, as in previous years; the angles for Occultations are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

ERRATA, Vol. VIII. p. 29.

Line 9 from bottom, for a_1/a read a/a_1. Line 6 from bottom, for $1/a$ read a.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 95. MARCH. 1885.

ANNIVERSARY MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, February 13, 1885.

E. Dunkin, F.R.S., *President*, in the Chair.

Secretaries: E. B. Knobel, Esq., and
Lieut.-Col. G. L. Tupman.

The Minutes of the last Anniversary Meeting were read and confirmed.

Mr. Lecky read the Report of the Auditors.

Mr. Knobel read the names of the Associates and Fellows who had died during the year. The obituary notice, by Dr. Routh, of Dr. Todhunter was also read, and some selections from other parts of the Report of the Council; but as these will be printed *in extenso* in the 'Monthly Notices,' we have not thought it necessary to reproduce them here. As the Presidential Address will also appear in full, we only give a very brief abstract of it.

The President read his Address to the Society, explaining the grounds upon which the Council had awarded the Gold Medal to Dr. Huggins. He prefaced his remarks by pointing out that the list of the Medallists of the Society showed that no branch of astronomy had been neglected, and that the awards had been made without respect to nationality, and with all impartiality. After alluding to the former occasion (1867) on which Dr. Huggins, in conjunction with Dr. Miller, had received the Medal of the Society, the President said that though Dr. Huggins did not claim to have originated *the idea* of determining the motion of stars by means of the spectroscope, yet he was the first to reduce it to practice, and to show that trustworthy results were attainable. After detailing the history of the theory of the displacement of the lines in the spectrum of a heavenly body and recounting the difficulties Dr. Huggins had met and overcome, the President showed how fully the results Dr. Huggins had obtained had been confirmed at Greenwich and other observatories. The President next pointed out how thoroughly Dr. Huggins had led the way in photographing the spectra of every order of heavenly body, stars, planets, comets, and nebulæ, and he expounded at great length the results obtained from each. How successfully Dr. Huggins had overcome the difficulties of these researches was shown by the definition of many of his photographs of stellar spectra, upon which, though the entire spectrum from G to P is but ½ inch in length, fourteen lines may be counted between H and K.

The President then delivering the Medal to *Dr. Huggins*, addressed him in the following terms :—

"Dr. Huggins,—I have great pleasure in presenting you with this Medal, which has been honourably awarded to you by the Council. I trust you will accept it as the highest acknowledgment of your valuable services to our science which it is in their power to bestow. This is not the first time that your devotion to astronomy has been recognized from this Chair, and I hope it is not the last; for we still find you carrying on with success your important and delicate researches. May your health be long preserved, and may astronomy long continue to receive the benefit of your talents, which I have no doubt will be the means of still further adding to our present knowledge of the constitution of the universe."

Lord Crawford. I beg to propose that the Report be adopted, and that it be printed and circulated in the usual manner, together with the Report of the Auditors and the President's Address.

Professor Forbes. I have much pleasure in seconding the motion.

The motion was put and carried.

Mr. Waters. I rise, in accordance with notice I have given to the Council, to move a resolution asking the Council to take into consideration the alteration of Bye-law 44. The suggestion I have to make is, that we should alter the time of holding our Ordinary Meeting from 8 o'clock to 5 o'clock in the afternoon. I believe the present hour was fixed upon about 50 years ago; and I think it will be admitted that now we may, at any rate consider, whether a more convenient time may not be fixed. The evening Meetings are very inconvenient to many Members who live in the country, who are obliged to stay in London for the night, or be exposed to a late train-journey home. The Meetings are also not particularly convenient to those who live in London, and who are engaged, or ought to be engaged, elsewhere at that hour. Moreover, I think the evening Meetings prevent the attendance of some of our most eminent, if older, Fellows who might be inclined to attend if the Meetings were held in the afternoon. Among the minor reasons for a change I may mention that the Meetings at the Royal Institution are held on Friday evenings at the same time as ours, and a great many of our Fellows are Members of the Royal Institution; and that is a point that might be considered. If this Society should decide on any change, we may perhaps be able to judge of what the effect will be, inasmuch as the Royal Society some years ago made a similar alteration, and the result has been a considerable increase in the attendance; and those Fellows of that Society with whom I have had an opportunity of speaking have expressed their satisfaction at the change. I will not occupy the time of the Meeting longer, and will only add that I have brought the matter forward not necessarily for settlement at the present Meeting, but more with the idea of eliciting an expression of opinion and bringing it forward again for future consideration. With that object I will propose: "That Bye-law 44 be altered by substituting '17 hours' for '8 o'clock'."

Rev. A. Freeman. I have very much pleasure in seconding that. When I resided at Cambridge I found it inconvenient to come to town for an evening Meeting, because it was next to impossible to get back the same evening; and now that I live forty-five miles from London, I find it still more inconvenient. I hope we shall not shrink from coming to a division, for I came expressly to record my vote; but if I cannot do so in four minutes I shall not have the opportunity of supporting the resolution. (Laughter.)

Mr. Ranyard. I have noticed that at these afternoon meetings of the Society, the attendance is always somewhat different from that at our evening meetings; and it seems to me rather unfair that the Fellows who are able to come here in the afternoon should decide for those who come here in the evening. As far as I know, a great many Fellows are occupied during the day, and would not be able to come in the afternoon. And I think, when a rule has been carried out for half a century, there should be some very decided majority of feeling shown by the Society for a change; and therefore, unless there is such a decided majority here, and confirmed also at an evening meeting, I think it would be unwise to change our course. *Mr. Ranyard* also criticised the wording of the motion. He objected to the expression "17 hours," and thought that the Royal Astronomical Society ought not to throw their weight in favour of the somewhat pedantic language of the new nomenclature of time, without very good cause.

Mr. Knobel. I should like to place a few facts before the Society with regard to the effect of Mr. Waters's proposal, so that the Fellows may understand what it involves. It is obvious that, in conducting the affairs of the Society, the meetings of the Council and of Committees must take place on the day of the ordinary meeting of the Society; and therefore we must take it for granted that, if the change is made, the meetings of the Council and of Committees will take place before the proposed meeting at 5 o'clock. The Council meetings usually occupy from two to three hours. At our December meeting, the Council have to consider the claims of nominees for the Medal and the preparation of the Council list, and that always takes a great deal of time, and three hours must be allowed for that purpose. This would necessitate the Council meeting at 2 o'clock, in order that it might conclude by 5 o'clock. At several of our meetings during the year, Library Committees have to be held, which take at least three-quarters of an hour; and therefore the Library Committee would have to meet at a quarter past 1 o'clock, in order that the members may be present at the 2 o'clock meeting of the Council. At the proposed 5 o'clock meeting, the business would go on until 7 o'clock at the earliest; and there will be a quarter or half an hour's business afterwards with the Assistant Secretary, arising out of the General Meeting; and therefore the business would not be over before half-past seven: that is to say, that the President and Officers of the Society would be uninterruptedly at work, without any time for refreshment, from a quarter past one until half-past seven.

Lord Crawford thought that five was too early, because few business-men would be at liberty at that time. If Mr. Waters had suggested 7 o'clock, he would have been with him.

Mr. E. J. Stone. As a resident in the country, I should prefer five, the adoption of seven would make little difference. Individually, I am in favour of the change, but I do not think this meeting ought to take the responsibility of making it. I should propose that a circular be sent round to all the Members for an expression of opinion, and no change made unless a large majority decided in its favour. With respect to the adoption of the term seventeen or nineteen hours, I look upon it as a little premature. It is very doubtful indeed, as far as I can see, whether it really will be adopted by astronomers, and it should be adopted only after it has been deliberately accepted by a large majority. For myself, I should be willing to adopt it, if some definite epoch was fixed upon at which to begin the new system.

The Astronomer Royal thought it a pity to mix up two distinct questions. Any discussion on the "new time" was premature, inasmuch as no *official* notification of the proceedings at Washington had yet been received. With regard to the other matter, he personally thought the change would be beneficial, but it should not be made without due consideration. The Meetings of the Royal Society, which were held at 4.30 P.M., were better attended than when they had been held at 8 P.M. With regard to Mr. Knobel's objection, the Council Meetings were almost always over by 5.30 at present, and he thought it feasible to hold the Council Meetings before the 5 o'clock meeting of the Society, and to have a short interval for tea before the meeting instead of after it. That was the practice of the Royal Society, and their Council meetings only occupied about half the time they used to do formerly. Then the Library Committee need not be held on a day when there is a very long Council meeting. He therefore proposed that the matter be left to the consideration of the Council, and for it to endeavour to obtain the general opinion of the Society on the subject. (Hear, hear.)

Mr. Jackson. I have great pleasure in seconding that motion, and at the same time I can show the Society a very curious clock-face. The Italian painters were satisfied with dividing the clock-face into four parts—

Mr. Jackson was proceeding, but the *President* ruled that the subject was not relevant to the present discussion.

Mr. Waters. I shall have great pleasure in agreeing to the suggestion of the Astronomer Royal. I brought the matter forward merely in order to elicit opinions on the subject.

Mr. Chambers. I trust this question will not be simply shelved, and should like to suggest that reply post-cards should be sent to each Fellow with some such question as this:—"Are you in favour of an alteration of the hour of meeting? and if so, would you prefer 5 or 7 o'clock?" In that way you will be able to gain a fair statement of the wishes of the 500 Fellows of the Society, which cannot be done at any meeting, owing to the wide area over which they are scattered. Personally, I am strongly in favour of an alteration to 5 o'clock; but unless a poll is taken in some such way as I suggest, I do not think it would be possible to learn the wishes of absentees.

Professor Adams. Might it not be possible to try the experiment of a change for two or three meetings? and then we should see whether the meetings would be better attended.

Lord Crawford. I do not think that would be possible, because the bye-laws say we shall hold the meeting at a definite time. I quite agree with Mr. Chambers that if the thing is to be done at all it should be done thoroughly; and if we drop the subject now, and the Council will undertake to ventilate the matter thoroughly among the Fellows, that will be a proper course to pursue.

Professor Pritchard. I could second the motion of Mr. Chambers. It seems to me a practical suggestion.

The President. I should like to say that the Royal Society, before they made the alteration, sent out circulars to each Fellow of the Society, and only one-half of them was returned, but a majority of that half was in favour of altering the time from 8 to 4.30 P.M.

Mr. Knobel. Does the Council of the Royal Society meet before their regular meeting?

Mr. Ranyard. Yes.

Professor Adams. There would be one advantage attending a change. It would make the Council use their time better, and not dawdle so much over the business. (Laughter.)

Some discussion took place as to whether Mr. Chambers's motion could be taken as an amendment to Mr. Waters's motion; and ultimately Mr. Waters formally withdrew his motion. Mr. Chambers proposed his in the following terms:—"That the Council be requested, within the next eight weeks, to send a reply post-card to each Fellow, asking for a reply to the question whether the Fellow receiving the same is in favour of or against an alteration of the hour of meeting from 8 to 5 o'clock."

Mr. Stone seconded the resolution, and the *President* put it to the Meeting, and declared it carried.

Mr. Ranyard. If that question is disposed of, there is another matter I should like to mention. Mr. W. S. Franks has sent out a notice to some of his friends, who have signed it, and it is this:—"We, the undersigned Fellows of the Society, respectfully request the Council to consider the advisability of altering the Bye-laws, so as to enable Fellows who cannot attend the Annual General Meetings of the Society to vote at such meetings by proxy."

Mr. Knobel. The requisition, which Mr. Ranyard has read, has been received by the Council to-day, and they will take it into consideration at their next meeting; I think it is out of order to bring it up now.

Mr. Ranyard considered that he was not out of order. He was not proposing an alteration of a Bye-law, but only wished the sense of the Meeting to be taken on the important question raised by Mr. Franks.

Mr. Knobel called attention to Bye-law 52, which provided that the requisition should be considered by the Council. Failing their action, a special General Meeting might be called.

Mr. Ranyard said the Meeting could make any recommendation it might like to the Council.

Mr. Common replied that if the question involved an alteration of a Bye-law, it was distinctly out of order to bring it forward.

Mr. Ranyard said he was proposing no alteration, but only to recommend the Council to make an alteration.

Prof. Adams said that a particular mode of procedure was laid down in the Bye-laws, and the method Mr. Ranyard was proposing was exactly the reverse.

Mr. Ranyard replied that the sole object he had was to secure discussion of the question. *The President*, however, ruled that he was out of order; and *Lord Crawford*, whilst expressing his sympathy with Mr. Franks's proposal, pointed out that Mr. Ranyard had not a leg to stand upon. A discussion now would only prejudice the decision, which would have to be arrived at probably by a special General Meeting. If the Council refused to consider the question, the requisitionists could demand that a special General Meeting be called.

Mr. Chambers, Dr. Mann, and Mr. Waters were then appointed Scrutineers, and the Meeting proceeded to the election of Officers and Council for the ensuing year. The following Fellows were elected:—*President*: Mr. Dunkin. *Vice-Presidents*: Profs. Adams and Cayley, Dr. De La Rue and Mr. Stone. *Treasurer*: Mr. Common. *Secretaries*: Mr. Knobel and Lt.-Col. Tupman. *Foreign Secretary*: Dr. Huggins. *Council*: Capt. Abney, Sir G. B. Airy, Prof. Pritchard, Major-Gen. Tennant, and Messrs. Capron, Christie, Downing, Glaisher, Hind, Knott, Maunder, and Ranyard.

The following gentlemen were duly elected Fellows of the Society:—Rev. Edward Allen, M.A., Rev. John Burgess, and P. E. Dove.

The meeting adjourned at $6^h\ 0^m$ P.M.

Liverpool Astronomical Society.

At the Monthly Meeting held on February 9, the President in the chair, Mr. W. H. Sharp detailed some interesting experiments he had made with a view of redetermining the density of the Earth. The method employed was a modification of the plan adopted by Cavendish, but, instead of a pendulum hung horizontally from the centre, he had used one swung after the fashion of a gate. Friction had been reduced to a minimum by the substitution of needle-points for hinges, and a centre or zero of oscillation was obtained by adjusting the lower centre a little out of the vertical.

Mr. Joseph Baxendell, Jun., wished to acknowledge the kindness of Mr. George Knott, F.R.A.S., in furnishing him with the dates of 11 maxima and 9 minima of the variable star R Ursæ Majoris. From these he deduced a difference between the calculated and observed times of maxima of 12·46 days, the greatest difference being 24·92 days, and of the minima 8·17 and 21·92 days respectively. In a paper on the variable star χ Cygni, Mr. J. E. Gore, F.R.A.S., said this interesting variable had lately passed through a maximum of about average brightness. His observations had fixed the date as Nov. 23, 1884, when he thought the star was preceptibly brighter than 8 Cygni. The average period seemed to be about 406·5 days, but, according to Schönfeld, the period was slightly lengthening. The observations had been made with a powerful binocular; but the star was above 6 mag. or visible to the naked eye from Nov. 2, 1884, to Jan. 6, 1885, a period of 65 days.

Mr. G. T. Davis had noticed that a fifth star had lately become visible in the trapezium of Orion, with his 3¾-in. Wray. This star was *s.f.* the brightest star, and was probably identical with Herschel's 6th star, which Webb thought would prove variable.

In a description of the Lunar Alpine Valley, Mr. Elger, F.R.A.S., said the scenery in this neighbourhood was extremely difficult to sketch, especially under a high sun, when alone the conditions prevailed which permitted the complicated details to be clearly seen and interpreted from end to end. It was only when far removed from the terminator that its V-shaped outlet to the Mare Imbrium could be traced to advantage, or the flask-like expansion with the constricted gorge, which led up to it from the north-west, satisfactorily observed. At other times these features were always more or less concealed by the shadows of neighbouring heights. The details of the upper or more attenuated end of the valley were, however, seen best under a setting sun, when many striking objects came to light, of which few traces appeared at other times.

Mr. A. S. Williams, F.R.A.S., called attention to a singular variation in the brightness of the lunar crater Boscovich. The crater itself was known to students as one of the darkest and most obscure objects on the Moon, being only half a magnitude brighter than the black shadows. Some months ago he had, however, seen it distinctly brighter, but lately it had again darkened until it was almost as black as the photometric measures made it. From the length of time over which the changes extended, they could not be produced by variation in the angles of illumination, but would seem to be due to some physical cause.

The Secretary said that this seemed to be only another instance of the want of a comparatively cheap and reliable map of the Moon, and he was glad to say the publication of such a map was now in contemplation by the Society. It had at first been suggested that the map should include all detail observable with a 9-in. aperture; but he thought at present this would be too expensive an undertaking, though they might attempt it later on. Perhaps a map about 3 feet in diameter, and with markings within the scope of, say, 4-inch telescopes, would be most suitable. The matter was, however, still in abeyance and he would be glad to receive suggestions.

Mr. W. F. Denning, F.R.A.S., thought the idea a good one and hoped it would not fall through, as the Society numbered many who had given especial attention to selenography. He was of opinion that something on a less extensive scale than that originally suggested might be attempted.

The President announced that a 9-in. reflector had been generously placed at their disposal by Mr. Calver, F.R.A.S. It was already at work on Birmingham's red stars, and he hoped shortly to give a favourable account of its performance.

Nine gentlemen were elected members of the Society.

Jupiter and the Relative Powers of Telescopes in Defining Planetary Markings.

THE Annual Report of the Director of the Dearborn Observatory for 1884 contains the usual valuable micrometric measures of certain selected markings on Jupiter, and the rotation periods derived from them. Prof. Hough's results in reference to the red spot and equatoreal white spot are nearly the same as those obtained at Bristol, as will be evident from the following comparison:—

Rotation Period of Red Spot.

	h m s		h m s
Opposition of 1882–3	=9 55 38·4	Opposition of 1882–3	=9 55 39·1
Opposition of 1883–4	=9 55 38·5	Opposition of 1883–4	=9 55 39·1
Observations at Chicago.		Observations at Bristol.	

The period has now evidently ceased to increase.

Rotation Period of Equatoreal White Spot.

	h m s		h m s
Opposition of 1882–3	=9 50 9·8	Opposition of 1882–3	=9 50 9·6
Opposition of 1883–4	=9 50 12·7	Opposition of 1883–4	=9 50 12·1
Observations at Chicago.		Observations at Bristol.	

The period has been rapidly increasing. The results in this case are quite as accordant as for the red spot. It is curious that Prof. Hough found the period for the former exactly $9^h\ 50^m\ 9^s{\cdot}8$ during each of the three apparitions prior to that of 1883–4, when the motion of the spot became greatly retarded. At Bristol the observed retardation has been more gradually developed, though showing some irregularities during the several years.

While acknowledging the value of the micrometric results given in the Report from the Dearborn Observatory, it must be confessed that some of the statements made by Prof. Hough relative to Jovian phenomena are utterly inconsistent with the results of English observers. On page 9 he states:—"During the past opposition it was alleged in foreign journals that the spot had lost its outline, and become merged in a faint belt on the following end. This statement is entirely erroneous, as it was subsequently seen on various occasions with the Chicago telescope, entirely separate and distinct from any belt, and presenting the same outline that it did in 1879."

Prof. Hough evidently refers to the notes and drawing in the 'Observatory' for March, 1884, p. 71; and as the point is an interesting one, I am induced to quote the following observations:—

By Mr. N. E. Green, London. (18-inch Reflector, powers 250 to 400.)

1884, Jan. 6, $10^h\ 0^m$.	The coalescence of the spot and belt to the south not observed.
Jan. 8, 10 40	Connection of spot and belt intimated.
Jan. 24, 13 10	Connection evident.
Jan. 28, 7 40	Connection of spot and belt broad and unmistakeable.

On Feb. 2, $7^h\ 15^m$; Feb. 6, $9^h\ 30^m$ and $10^h\ 30^m$; Feb. 10, $12^h\ 5^m$ and $12^h\ 40^m$, the connection of the S. *f* end of the spot with the grey belt on its S. border was noted as very decided. On Feb. 11, $7^h\ 50^m$ and $8^h\ 40^m$, it seemed to have become fainter and reduced to a narrower strip. There was no doubt whatever as to the ligament running from the spot to the belt, not only as to form, but colour, which was a faint reddish grey. It was most evident on Jan. 28, and had become fainter by Feb. 11, after which no further observations were made.

By Mr. J. W. Backhouse, Sunderland. (4¼-inch Cooke Refractor.)

"1884, Jan. 25, $9^h\ 34^m$ (powers 180–350). The red spot plain again, though its form is not plain; its *f* end seems to slant southwards, and is joined to a belt south of the red spot; at any rate, I cannot see any space between them. But the spot is distinctly red, while the belt to the south is not, nor any other belt but the southern equatoreal belt, which is very red, especially the darkest (S.) part, which is very strongly tinted crimson. Air very good.—$10^h\ 30^m$. The red spot still seems joined to the belt S. Air not quite so good."

very much smaller instruments. A 6½-inch With or Calver mirror would show them all without any difficulty.

Sir W. Herschel seems to have recognized the non-utility of large instruments in the observation of bright planets, for he wrote as follows:—"In the course of these observations [on the belts of Saturn] I made ten new object specula, and fourteen small plain ones, for my 7-foot reflector [of $6\frac{3}{10}$-inch aperture], having already found that with this instrument I had light sufficient to see the belts of Saturn completely well; and that here the maximum of distinctness might be much easier obtained than where large apertures are concerned." (Phil. Trans. vol. lxxxiv. p. 51.)

There is a chart of Mars by Profs. Harkness and Hall in the 'Monthly Notices,' vol. xl. p. 13, derived from eight drawings with the 26-inch Washington refractor, between Aug. 18 and Oct. 18, 1877, just when the markings ought to have been visible with maximum effect. A good 4-inch refractor would show all the detail included in this chart! Look at the mass of complicated features seen by Schiaparelli with a refractor of 8 inches aperture at the much less favourable opposition of 1879 and 1881–2. The drawings of Mr. Dawes and Mr. Green are also far and away more comprehensive, though founded on views with much smaller instruments than the great refractor at Washington.

Mr. R. S. Newall, who possesses perhaps the best instrument in England, the 25-inch Cooke refractor, now mounted at Gateshead, remarked, in a recent letter to me, that "atmosphere has an *immense deal* to do with definition. I have only had *one fine night* since 1870! I then saw what I have never seen since."

Many other statements of similar tendency with regard to the extent by which large apertures are affected by atmospheric undulations might be quoted, and there is no doubt such instruments are rendered practically useless in some departments of observation from this cause. While possessing an immense superiority of light over smaller telescopes, they are rendered ineffective by inferior definition. Imperfections of figure will not account for this, for the large telescopes of Rosse, Herschel, Lassell, Alvan Clark, Cooke, and others must, we know, have been constructed as nearly perfect as human hands could make them; but the real cause of disappointment lies in atmospheric currents, and their far greater apparent activity with every increase of aperture.

More attention should be given to instruments of say from 6 to 10 inches, and the means of yet further enhancing their performance. A great and unnecessary loss of light is occasioned by the use of two lenses in the eyepiece *. I have for some time employed only the single (eye) lens of my Huyghenian eyepieces, and find not only a great saving of light, but an improvement in definition. The same thing was commented on by Sir W. Herschel more than a century ago in the 'Phil. Trans.' vol. lxxii., in these words:—"I have always from experience persisted in refusing the assistance of a second glass, which is sure to introduce errors greater than those we would correct. I have tried both the single and double eye-glass of equal powers and always found that the single eye-glass had much the superiority in point of light and distinctness." The field is smaller and only available near its centre, but on the whole it is an immense improvement. What we essentially require to obtain is the greatest amount of light and perfection of figure possible from instruments of moderate aperture. Prisms, flats, Barlow lenses, and diaphragms all occasion the loss of valuable light, and it would be well if they could be dispensed with altogether. It will be an interesting point to determine what limit of aperture affords illuminating power and sharp definition in the most perfect degree as regards planetary markings.

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* Dr. Ball, in his 'Elements of Astronomy,' p. 14, is somewhat inexact in dealing with this question. He says:—"There is so much distortion produced by a single eye-lens in those objects which lie near the margin of the field of view that the compound eyepiece, consisting of two lenses, is generally preferred. By its aid the same magnifying-power is retained while the distortion is greatly diminished. On the other hand, the introduction of a second lens is of course attended with some sacrifice of light." The fact is, the

In this paper I hope it will not be understood that I have attempted any undue disparagement of large instruments. I am far from subscribing to Dr. Kitchener's opinion that "Immense telescopes are only about as useful as the enormous spectacles which are suspended over the doors of opticians!" In certain departments of research large apertures are absolutely required, and have performed valuable work utterly beyond the capacity of moderate instruments. W. F. Denning.

Bristol, 1885, February 2.

The Proposed Change of the Astronomical Day.

Two of our highest astronomical authorities have recently pronounced their verdicts upon the decision of the Washington Prime Meridian Conference with respect to the astronomical day, and in each case the verdict is an adverse one. The first, Herr Foerster*, is of opinion that the so-called "universal" day should coincide with the astronomical; but he holds that the general public should give way to the astronomers, not the reverse, and that consequently the universal day should commence, as the astronomical day does at present, at mean noon. He says that the "significantly small majority" which carried the resolution recommending that Greenwich midnight be the point of departure was at the previous conference at Rome a minority, and that therefore it behoves them to justify

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magnifying-power of the negative eyepiece is greatly lessened by the addition of a second lens. Employing only the eye-lens of four Huyghenian eyepieces on my 10-inch reflector, I get powers of about 128, 252, 475, and 641. Adding the field-lens the numbers are reduced to 97, 145, 290, and 327. In the case of the positive (Ramsden) eyepieces, however, the powers are slightly less with the single eye-lens than with the ordinary pair of lenses.

* Astron. Nachr. No. 2643.

the rejection of the views expressed at the earlier conference. The plea urged at Washington was that it was intolerable to demand the European commercial public to change the date in the middle of the business day; yet, despite all that was said on the subject at Rome, astronomers and navigators are now asked to give up their commonly accepted and well-founded mode of reckoning. Herr Foerster further considers that the mistake has arisen from supposing that the public generally have anything to do with the question of Universal Time. The entire matter could and ought to have been argued on astronomical grounds alone. He is also of opinion that it will be difficult to get the travelling public to adopt it, and that it is quite unnecessary in telegraphy; so long as the reckoning of time proceeds from 0^h to 24^h, every necessity of the case is met. He concludes a long article by declaring that "There is no forcible reason for choosing any other basis than the astronomical day at Greenwich for the universal astronomical and nautical day; and, further, it still remains necessary to choose a simpler and more favourable plan than the adoption of a universal day, which offers no advantage other than its coincidence with the English civil day." Herr Foerster also states that Herr Tietjen will not employ the new universal day in the 'Berliner Jahrbuch.'

In reviewing Herr Foerster's article, of which we have attempted thus to give an abstract, we feel ourselves obliged to dissent from not a few of his conclusions. And first of all we would point out that the Roman conference was expressly and professedly tentative only, that its resolutions were put forward not as recommendations, but as points for consideration, and that it was most explicitly stated that they were in no way to bind the action of the special conference called in Washington the following year. Considering this fact, and also that the scientific world in general, and the members of the Conference in particular, had a year in which to weigh the arguments *pro* and *con* the Roman proposition, it seems scarcely fair to insist much on the point that the view held by the majority at Washington, which was not small as Herr Foerster alleges, had been held only by a minority at Rome. The more legitimate inference would surely be that the earlier decision was framed upon a hasty and insufficient consideration of the facts of the case.

But we feel sure that Herr Foerster is mistaken in regarding the question either as a purely astronomical one, or as one in which the public convenience is a secondary matter. If the question had been simply an astronomical one there would have been little need for a conference. There is already a tendency to adopt Greenwich mean time in astronomical reckoning quite sufficiently strong to have produced ultimate uniformity; indeed it has already been accepted in the case of astronomical telegrams. But the development of commerce and the extension of telegraphs and railways were already rendering the existing confusion of systems intolerable; and the United States had taken a remarkable step towards securing simplicity and order by the adoption of a universal minute for local time. It was confessedly largely to meet this growing need that the Conference was called, and it would have stultified itself had it put the public convenience on one side as a matter of no importance. Further, had the Roman proposal been ratified, what would probably have happened would have been that, whilst astronomers retained their present reckoning, business men would have refused to adopt a method so foreign to their habits and so little adapted to their convenience, and the net result of the Conference would have been zero.

We think there can be no doubt that in recommending midnight for the point of departure of the universal day the Conference were undoubtedly in the right so far as regards the *civil* employment of time. But is it necessary that the *astronomical* day should be identical with the *civil* day? Prof. Newcomb, the second authority alluded to, thinks not, and he puts forward his views in a brief but very clear paper appearing in the 'Monthly Notices' for January. He dwells strongly on the confusion which the alteration in the astronomical day would introduce into our computations and ephemerides, and points out that there would be a break of half a day in the continuity of astronomical dating. On the other hand, the present astronomical day has been used side by side with the present civil day for over 200 years without any confusion arising; the general public have not known of the difference and astronomers have been perfectly accustomed to it. Prof. Newcomb concludes by recommending that at least the change be postponed for a while to a more fitting epoch than the present. "The beginning of the coming century," he dryly adds, "would be a very good epoch, and would allow about the right time for consideration."

There can be no doubt that Prof. Newcomb's arguments have great force, though we are personally inclined to think he somewhat exaggerates the difficulty of the change, and he neglects to note one great advantage which would be secured, viz. the adoption of a common standard of time amongst astronomers the world over. Are not the present diverse standards of astronomical time quite as fruitful of trouble and error as the proposed change would be, except, perhaps, just at the very moment of making it? But there are several considerations which seem to point strongly to the advisability of the delay which Prof. Newcomb recommends. First, we do not yet know if the universal day will meet with any public favour. Should this not be the case astronomers would have altered their mode of reckoning without any compensating advantage. Next, it is impossible for the change to be made in observatories until it has been previously made in the great ephemerides, and these are published for several years in advance; and, further, as we have seen, the editors of two of the most important have pronounced against the change. Until the chiefs of the principal Nautical Almanac offices have agreed to introduce the change no forward step can be taken. Thirdly, it is much to be deprecated that attempts to introduce the new system, otherwise than in internal organization, should be made at isolated observatories here and there. The change should not be made until astronomers are unanimous upon the subject, or, at least, until those favouring the change are so greatly in the majority as to render the dissent of the minority a matter of no consequence. The change should then be made, as Prof. Newcomb advises, "at some common fundamental epoch" adopted "by an arrangement among the astronomers of the world." Failing these precautions the result of following the recommendation of the Washington Conference would not be the removal of anomalies which it was designed to effect, but the introduction of a "confusion worse confounded." **The Editors.**

The Lick Observatory.*

It may be of interest to give a brief account of recent work at the Lick Observatory. It is difficult to convey an adequate idea of the excellence of the site, and of the thorough way in which the Observatory has been constructed by the Lick trustees, without the aid of illustrations.

The most recent addition to the observatory is an exquisite meridian-circle by the Repsolds, of Hamburg, and the house to contain it.

The meridian-circle house is forty feet square, with the wall double throughout. The inner wall is of California redwood, whic is almost incombustible, and beyond this is an air-space of 2 inches completely round the building, on the north side as well a on the others. The outer frame carries a louvre-work of galvanize iron, which completely prevents the Sun from striking any part o the building proper. The ceiling is flat, of wood, and a very large air-space communicates with the room itself through wire netting which covers the sides of the slit. Complete access to any part of the air-spaces, both of the walls and of the roof, is provided. On the west, the room opens into a ventilating tower, two stories in height, which also adjoins, and is connected with the house for the meridian-transit instrument, which lies still further to the west. The design of this construction is to keep the temperature of the two houses and of their air-spaces precisely the same as that of the external air, and it is probable that this object has been practically attained. The new Repsold meridian-circle, which has just been mounted, is of six (French) inches aperture, and is essentially of the same design as the meridian-circle of Strassburg, and of the smaller circles of Williamstown and Madison.

It has north and south collimators of six inches aperture, and an east and west collimator of three inches. The three six-inch objectives were made by Alvan Clark and Sons.

There are two circles, each divided to two minutes, and read by four microscopes (magnifying forty diameters) to one second.

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* From the 'Sidereal Messenger,' Vol. iii. p. 301.

Two extra microscopes of seventy diameters serve for the determination of the division-errors. One of the circles is fixed to the axis, and will serve first as a setting-circle, so that the same setting will always hold good for the same object, which is a great convenience, and, secondly, this fixed circle will serve for star observations in the usual manner. The other circle is movable around the axis by a wheel and pinion. It will serve for fundamental observations when the division-errors of each one-degree mark and the errors of the two-minute marks for four single degrees, ninety degrees apart, are once determined. It can also be used to eliminate the accidental division-errors, by turning it each day through some number of minutes, as 7, 11, 13, &c., so that successive observations of the same star will fall on different divisions. The complete division of the two circles also allows of a determination of the flexure of the circles, &c. The large size of the collimator objectives permit the pointing of one collimator on the other with a precision equal to that of the pointing of the telescope on the collimators, and will serve for very precise determinations of the horizontal flexure. Complete provision is also made for reflection observations. A *mire*, or meridian mark eighty feet distant, may eventually replace the south collimator, whose objective is to be fitted to an equatoreal mounting, also by Repsold. A *mire* may also be mounted some eighty feet west of the circle, on a pier within the main building. Altogether this circle is the most perfect of its class in the United States, if not in the world. It has been inspected by Prof. Auwers and Dr. Krueger, and pronounced by them to be satisfactory in all particulars. Its site is especially good. The elevation above the sea is 4200 feet, and for six or seven months of the year every night is clear. Half of the remainder of the time is also clear. It is plain that especial advantages will follow from a site which permits of continuous observations. The piers are built in the most substantial manner, of hard brick laid in cement, and their foundations are to be enclosed in air-tight boxes. The astronomical advantages of this perfect instrument, mounted in such a site, in so careful a manner, cannot be over-estimated. The construction of this meridian-circle house, of a double-house for the observers, and the finishing of the interior of the main building have occupied the whole of this season.

The entire observatory may be said to be complete with the exception of the 36-inch equatoreal and of the dome to contain it. The flint disk for the objective of the 36-inch has long been completed. The crown disk has presented unexpected difficulties, no less than nineteen unsuccessful disks having been cast. It is hoped that a perfect disk may be had in a few months, and that the whole objective will be made and mounted in 1887 or 1888. The construction of the dome and of the mounting will be proceeded with as soon as the crown disk is successfully cast. The observatory is now a very complete one, possessing as it does a

12-inch equatoreal, a 4-inch transit, a comet-seeker, a 6-inch meridian-circle, and a photoheliograph, as well as a Repsold vertical-circle and a 6-inch equatoreal. The main building is practically complete and is perfectly adapted to its uses. Five very excellent clocks (by Hohwu, Dent, C. Frodsham, and Howard) and set of chronometers by Negus will soon be available. The beginnings of a large astronomical library have been made. A complete electrical system has been put in for connecting clocks, chronographs, &c., and there are only a few observatories in the world which are in better condition for astronomical work than the Lick Observatory now is, even without its chief instrument, the 36-inch refractor.

E. S. HOLDEN.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Occultation of Aldebaran in the Sixth Century.—Bliss, Astronomer Royal.

GENTLEMEN,—

I have been looking at Streete's 'Astronomia Carolina,' to which Mr. Johnson refers in his letter in your last number. The copy of that work in the Library of the British Museum possesses an interest to which it is worth calling attention. Very little is known of the personal history of Bliss, who succeeded Bradley as Astronomer Royal, and filled that high office for only about two years, as he died at the Royal Observatory on September 2, 1764. After trying for a long time without success to learn something about his early life, I found in this volume a note on the titlepage that it was "the gift of Sam. Essington to Nat. Bliss, A° 1718, Feb. 13." Thus is proved the early interest taken in our science by the then future Astronomer Royal; for as Bliss was born on the 28th of November, 1700, he had not long completed his eighteenth year at the date in question, which must of course be taken as O.S. On the back of the cover is his own signature, "Nath. Bliss, Coll. Pemb. Oxon;" and on the flyleaf, in an unknown hand, we have the following particulars about him, most of which have, I believe, never appeared in print:—

"Nathaniel Bliss was the son of Nath. Bliss, Gent., of Bisley in Gloucestershire. He was matriculated at Pembr. Oct. 10, 1716, proceeded B.A. June 1, 1719, M.A. May 2, 1723. In 1736 he was rector of St. Ebbe's, Oxford; afterwards Observer at Greenwich 1762, Savilian Prof. of Astronomy * 1763, and died 1764.

"He had a son, John Bliss, matr. of Ch. Ch. Oct. 24, 1740, being then 16, B.A. of Merton, probably a clerk or chaplain Mar. 11, 174$\frac{5}{6}$, M.A. July 7, 1747."

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* [This is a mistake. He was Savilian Prof. of *Geometry*, some years before he became Astronomer Royal. Dr. Hornsby was Savilian Prof. of Astronomy at this time.—W. T. L.].

The book afterwards came into the possession of W. Wales, of Transit of Venus celebrity; at least his name occurs in the middle of the titlepage, and a MS. note on the flyleaf, immediately below that to which I have referred, states that this is "the autograph of William Wales, a very celebrated mathematician, and master in Christ's Hospital."

With regard to the occultation of Aldebaran, or, as Streete calls it, Palilicium, a name which first applied to the Hyades generally, and afterwards restricted to the star in question, has long been obsolete, the Greek manuscript quoted by Boulliau (Bullialdus) does seem to me to imply that the unknown observer made his observation at Athens; at least the occultation of Venus quoted just before from the same manuscript is stated to have been observed at that place in the 192nd year of Diocletian, corresponding to A.D. 475. That of Aldebaran was, as Mr. Johnson remarks, on the 11th of March, A.D. 509, whilst Anastasius I. was Emperor. Boulliau thought that the place of observation must have been considerably east of Athens, because he concluded from his tables that there "Impossibile verò fuit Lunam occultasse oculum Tauri."

Yours faithfully,
W. T. Lynn.

Blackheath, 1885, Feb. 10.

Ptolemy's Catalogue.

Gentlemen,—

In the report you published of the last meeting of the Royal Astronomical Society, the account of my note on Ptolemy's Catalogue appears to be rather confused, and I would therefore solicit your kindness to give me a little space to explain more correctly the gist of the paper.

Ptolemy's Catalogue of Stars has an interest *sui generis* which can never fade, and it is obviously very desirable that all possible sources of information respecting it should be ransacked to enable us to possess a more perfect edition of it than we do at present. The difficulties which the Greek MSS. present are clearly put forward by Baily in Memoirs R.A.S. vol. xiii. p. 7, who also remarks on the great value of the Arabic versions as affording corrections to the possible errors in the Greek. I have therefore thought it not altogether useless labour to consult what Arabic versions (and their derivatives and translation) I can find, to elucidate some of the doubtful points in the Catalogue.

When we consider the number of centuries during which the Greek 'Almagest' lay dormant, and for a large portion of which period its Arabic translation was the only text-book of Arabian astronomy both Oriental and Occidental, we may well believe that no pains would be spared by the Arabs to hand down as correct a version of it as was possible.

The only translation from the Arabic with which we are acquainted is that made by Gerard of Cremona, who flourished [illegible]. I have examined three MSS. of his translation, [illegible] Museum of the fourteenth century (one of them [illegible] thirteenth century), and the third MS. of the fifteenth [illegible] belonging to the Earl of Crawford and Balcarres. The [illegible] Latin 'Almagest,' printed at Venice 1515, is undoubtedly Gerard of Cremona's translation. In the catalogue of stars [illegible] versions describe α Scorpii and α Orionis as "tendens [illegible]." This perfectly enigmatical expression has remained [illegible]d. It is apparently intended as the translation of the [illegible] equivalent of the Greek word ὑπόκιρρος, by which Ptolemy [illegible] that the star was 'yellowish' or 'reddish.' I have consulted four Arabic MSS., two of the 'Almagest' and two of Al Sûfi. In three of them, viz. in an Arabic 'Almagest' at the Bodleian Library*, and the two copies of Al Sufi, I find a sentence identical [illegible] to "tendens ad rapinam" in Gerard of Cremona; the verb and preposition are exactly translated by "tendens ad," but the noun, in the majority of instances in the Arabic, is an untranslatable word, probably from errors in copying.

Seeing that the astrological influence of the two stars in question (α Scorpii and α Orionis) was held to be evil and malign, that they were both supposed to *tend or to incline to revolt and rapine*, I have thought it probable that Gerard of Cremona found in his MS. the same untranslatable word which I have found in the MSS. consulted, and that in this dilemma he assumed that the word was the expression of the astrological significance of the stars and so gave it as "rapine." I am Gentlemen, yours faithfully,

Bocking, Braintree, 1885, Feb. 15. E. B. Knobel.

The Winter Meteor-showers.

Gentlemen,—

During November, with prevalent anticyclonic weather, the skies were for a long time overcast, so that it is not safe to speak very positively of the abundance of meteors; but it appears that some at least of the showers were poorly represented, notably the Leonids and Andromedes. At the end of October I saw a few of the [illegible], eleven in all, and not of any brilliancy; but then Moon and clouds interfered, and after that the true Taurids had it all their own way. Of these I saw 29 only, though I kept up [illegible] watches for them; and it seemed that the hourly number was [illegible]. One was equal to Jupiter, orange in colour at first, but turning to steel-blue afterwards, as is usual with this shower [illegible] of meteors. I heard of a brilliant one being seen [illegible] brighter than any star, which was probably a Taurid. This shower is a very diffuse one, sometimes lasting a long time, and [illegible] having several radiants. This season I observed a [illegible] in the middle of December, which seemed still to belong to it. The December Geminids were more of a success and fairly well observed, though no night was clear for long. About midnight on the 9th I watched for 2½ hours, and saw 36 meteors, of which 26 were Geminids. They very noticeably came in groups, occasionally 3 or 4 almost at the same moment, and then of similar appearance and magnitude. The radiant seemed to be at 111° +41°, a rather out-of-the-way place; but as the tracks are always so very short and no streaks are left, it is difficult to map many of them.

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* This very valuable MS. has been kindly lent to me by the Trustees at the [illegible] for the purpose of translating the Catalogue of Stars.

The 10th was quite overcast.

The 11th was partly clear, and I watched from 8 hrs. to 11 hrs., seeing 56 meteors, of which 46 were Geminids.

The hourly number in the clearer intervals rose to 24 Geminids, so it was evidently an unusually good shower. Only two were bright; but, unfortunately, the best one, at $8^h\ 33^m$, was just behind a small patch of cloud. The sky was lit up by most brilliant flashes, of an intensity quite equal to a half-moon, apparently of a greenish colour; but the meteor only reached the edge of the cloud as it died out. After this it was again cloudy and only a few more were seen, when the radiant seemed several degrees east of Castor instead of north of him, as at first.

In all, 86 Geminids were seen here, but not more than one or two left any visible streak, and only the largest one can have been other than white.

The January Quadrantids were quite obscured by cloud.

Yours truly,
H. Corder.

Great Baddow, 1885, Jan. 22.

Parallax-Hunting.

Gentlemen,—

With reference to Prof. Ball's remark, mentioned in 'The Observatory,' 1884, p. 335, as to the probability of there being in the sky no star with a parallax larger than a second, is it not very probable that the nearer stars may be moving with our Sun, and therefore would show but little proper motion? It is true they might have another motion independent of our Sun's, still that might not be the case; so, if such stars were small their parallax might long escape detection. There is likewise the possibility of a star chosen for comparison having the same motion as the star under observation for parallax.

T. W. Backhouse.

Sunderland, Feb. 11, 1885.

NOTES.

A Cause of Systematic Errors in Measures of Double Stars *.—M. Bigourdan has recently pointed out a possible cause of systematic error in observing position-angles. In the case of the Secretan Equatoreal of $0^m{\cdot}305$ aperture of the Paris Observatory, he finds that the diffraction-rings surrounding the images of the brighter stars are discontinuous; so that when the objective is turned through 180° round its optical axis, as in observing east and west of the meridian, there is sufficient difference in the relative appearance of the components of a double star to cause systematic errors in the observed position-angles. And, in fact, from a series of experiments which M. Bigourdan has carried out with this instrument, he finds that there are considerable differences in the position-angles measured under the same conditions by the same observer, and within a few minutes of each other; so that the line joining the stars makes much the same angle with the line joining the eyes (always horizontal) in the two cases, one being east of the pier and the other west. M. Bigourdan points out that the errors cannot be eliminated by determination of personal errors from observations of artificial stars, since in these latter measures the stars alone are moved and not the objective. It would be desirable, then, for observers who possess telescopes mounted in what is generally known as the "German" manner, and having such a defect as is described by M. Bigourdan, to have the tube of the telescope made movable round its optical axis, and to form each measure of position-angle from the mean of six made in six positions of the objective differing by 60°. As this would be practically impossible in the case of large instruments, M. Bigourdan suggests that observers, commencing a series of observations of double stars with telescopes of any considerable size, should turn the objective through 60° at the commencement of each year and remeasure the same pairs as in the preceding year, and to continue this process until each pair has been measured in the six positions of the objective.

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* Bulletin Astronomique, Tome i. p. 439.

THE "NEW TIME."—We extract the following from the 'Pall Mall Gazette,' of Feb. 5, 1885, for the benefit of those of our readers who may not have seen it:—

"Every one who knows the twenty-four-hour clock outside Greenwich Observatory, which has been the delight of so many generations of children and holiday-makers, will be surprised at the following statement in a recent number of a French paper:—

"'During the night of the 31st of December, 1884, and the 1st of January, 1885, the old clock struck twelve o'clock—that is to say, midnight—and the new clock, which had been placed a quarter of an hour previously by the side of the old one, replied by striking 24 o'clock. This new arrangement has thrown the whole neighbourhood into such disorder that the authorities have all the trouble in the world to preserve the new timepiece from stones and explosive bombs.'"

ASTRONOMICAL PRIZES.—Mr. C. S. Whittemore, Secretary of the Rochester Astronomical Society, informs us that Mr. Warner offers the following two astronomical prizes:—

First. Two hundred dollars for each and every discovery of a new comet made from February 1st, 1885, to February 1st, 1886, subject to the following conditions:—1. It must be discovered in America, Great Britain, and the Australian Continent and Islands, either by the naked eye or telescope, and it must be unexpected, except as to the comet of 1815, which is expected to reappear this year or next. 2. The discoverer must telegraph full particulars immediately to Dr. Lewis Swift, Director Warner Observatory, Rochester, N.Y. 3. This intelligence must *be communicated to no one* until a telegraphic acknowledgment has been received by the discoverer from Dr. Swift. Discoverers in Great Britain, the Australian Continent and Islands, West Indies, and South America are absolved from the restriction in conditions 2nd and 3rd.

Second. A prize of $200 in gold to any person in the world who will write the best 3000 word paper on the cause of the atmospheric effects (red light, &c.) accompanying sunset and sunrise during the past sixteen months. Essays must be exclusively sent prepaid to Dr. Lewis Swift, Director Warner Observatory, Rochester, New York, must be written in English, on one side of paper only, with ink, and must be in the simplest, untechnical phrase. Each competitor must sign a *nom de plume* to his essay, and enclose his real name and address in an envelope, superscribed with his *nom de plume*. The essays must be in Dr. Swift's hands by December 1, 1885.

THE OBSERVATORY OF YALE COLLEGE.—The report of the Observatory for the year ending June 19, 1884, has recently been issued. We regret to learn from it that Prof. Newton resigned his position as Director of the Observatory, on May 28. His successor does not appear to have been yet appointed. Dr. Waldo had charge of the Horological and Thermometric departments during the year. Dr. Elkin assumed charge of the Heliometer on January 15, 1884, and has commenced work with it. The principal lines of investigation, to which attention has been directed, are as follows:—1. The triangulation of the Pleiades. Very valuable information on the absolute and relative motions of the stars in this group will be afforded by a comparison of these measures with the similar ones made with the Königsburg Heliometer nearly half a century ago. 2. The determination of places of the Moon relative to neighbouring stars, suitable for the investigation of the parallactic inequality in the Moon's motion. 3. Measures of the diameter of Venus. 4. The general investigation of the adjustments &c. of the Instrument. From the experience in the use of the Heliometer which Dr. Elkin has had at Strassburg and at the Cape, we may be sure that the fine instrument belonging to the Yale College Observatory will be used to the best advantage, and we may expect results of exceptional value from the various series of measures now in progress. We regret, however, that no mention is made of observations for determination of stellar parallax, and we hope that other investigations will not be allowed to interfere with this work, the accomplishment of which it was generally understood was Dr. Elkin's main object and to which he was prepared to devote himself.

AIRY'S 'GRAVITATION'*.—A second edition of this unique work has recently been published. The original edition of 1834 had become very scarce, and of late years it was extremely difficult to obtain copies of it; so that, considering the value of the book to all students of celestial mechanics, whether mathematicians or no, the announcement of the issue of a new edition will doubtless be received with satisfaction by our readers. The work is remarkable from the fact that, without the introduction of a single algebraical symbol, it gives an explanation of the principal perturbations in the solar system, including within its scope the Lunar theory and the theory of Jupiter's satellites. The new edition appears to be merely a reprint, as the few *errata* occurring in the original issue are reproduced here. It is to be regretted that advantage was not taken of the opportunity to correct these.

THE MOTION OF HYPERION †.—In this paper Prof. Newcomb discusses the perturbations of Hyperion arising from the action of Titan. Prof. Hall has shown, from a discussion of his own and other observations of Hyperion, that there is a remarkable retrograde motion in the perisaturnium of this satellite, the period of which is about 18 years. Now at first sight this result appears to be inconsistent with the theory of gravitation. As, supposing that the motion of the perisaturnium of Hyperion is caused by the attraction of Titan, which is much the brightest and much the nearest to Hyperion of all the satellites of Saturn, it is easy to show, by the ordinary formulæ for secular variations, that the motion of the perisaturnium is direct; and assuming the mass of Titan to be $\frac{1}{10{,}000}$ of the mass of Saturn, its amount would be about 3° per annum. Instead of this direct motion we find from observation a retrograde motion of about 20° annually. This apparent inconsistency arises from a relation between the mean longitudes of the two satellites, in consequence of which the ordinary theory of secular variations is entirely inapplicable. This theory presupposes that the mean motions of the two bodies are incommensurable, so that to any one mean longitude of the one will correspond, in course of time, every mean longitude of the other. As a result of this, the conjunctions will be scattered through every part of the orbit. Now four times the mean motion of Hyperion is nearly equal to three times that of Titan. It follows that if the two are in conjunction at any moment of time, then after Hyperion has made three revolutions they will again be in conjunction at very nearly the same point. And if we assign to this point a slight motion, the conjunction will always take place on it. But four times the mean motion of

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* Gravitation. By Sir G. B. Airy, K.C.B., &c. Macmillan and Co., 1884.

† Astronomical Papers prepared for the Use of the American Ephemeris, Vol. iii. part iii.

Hyperion *minus* three times the mean motion of Titan equals $-0^{\circ}\cdot0515$. Hence the mean daily motion which we should assign to the conjunction point is $-0^{\circ}\cdot0515$, and its mean annual motion is $-18^{\circ}\cdot8$; which is sensibly the same as the motion of the perisaturnium of Hyperion found by Prof. Hall from observation. We therefore have the remarkable result that the conjunctions of Hyperion and Titan during recent years have taken place at or near the same point, which Prof. Newcomb subsequently finds to be the aposaturnium of the former satellite. Consequently during this period the theory of secular variations has not been applicable, and this is a "a new case in celestial mechanics." Prof. Newcomb proceeds, in the light of these results, to discuss the theory of the action of Titan on Hyperion, using the method of variation of elements. He finds the mass of Titan to be about $\frac{1}{12,500}$ of the mass of Saturn, and summarizes the results of his researches on the inequalities in the motion of Hyperion thus:—"All the sensible perturbations of Hyperion by Titan may be expressed as a function of the mean angular separation of the perisaturnia of the two satellites, of which the most probable value given by observation is

$$\pi-\pi'=185^{\circ}\cdot0+19^{\circ}\cdot5\,(t-1880\cdot0).$$

"We then have

Inequality of mean longitude, $\delta l'=-\ 2^{\circ}\cdot0\sin(\pi-\pi')$;
Inequality of perisaturnium, $\delta\pi'=+\ 10^{\circ}\cdot0\sin(\pi-\pi')$;
Inequality of eccentricity, $\delta e'=+0\cdot017\cos(\pi-\pi')$."

THE INNER SATELLITES OF SATURN *.—M. Baillaud has published the principal results of his discussion of the observations of the five inner satellites of Saturn made at Toulouse from 1876 to 1883. They may be exhibited as follows:—

Satellite.	Mean motion.	Eccentricity.	Annual motion of perisat.
Mimas	381°·9934	0·10 to 0·05	447°
Enceladus....	262 ·731	very small.	uncertain.
Tethys	190 ·69818	0·01±	70
Dione	131 ·53551	0·01±	10
Rhea........	79 ·69078	0·01+	—

The mean motions of the four last-mentioned satellites agree closely with those found from comparing the Toulouse observations with those of Jacob in 1857 and 1858, Herschel in 1790, and Cassini; so that the time of revolution of these satellites can be determined to a fraction of a second. M. Baillaud, however, points out that in the case of Tethys the epoch deduced from the Toulouse observations differs by about 3° from that given by Jacob's measures. But it appears that unless the mass of Mimas be a very small fraction of that of Tethys, the inequalities in the motions of the latter arising from the action of Mimas would be quite sensible, and hence may arise the discordance between the two determinations.

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* Comptes Rendus, Tome c. No. 4.

THE MEROPE NEBULA.—A Florida correspondent writes:—"This object is to me one of the steadiest and easiest objects amongst nebulæ. By turning the eyepiece half round, the nebula can be cut off from Merope, or Merope from the nebula. Between Merope and Alcyone the sky is black, and there is not a trace of glow around Alcyone. When the eyepiece has been turned so as to exclude Merope, the nebula has seemed, if anything, brightened; but when the bulk of the nebula has been shut out, there is but a very narrow strip of nebula left between Merope and Alcyone, not one thirtieth of its extent in the opposite direction. Nor does the nebula extend to Electra, or in that direction at all; it is limited to a roughly parabolic curve lying within a curve of similar shape, formed of small stars. But outside that stellar outline there is a star making almost an equilateral triangle with Merope and Electra, and distant from the edge of the nebula about the shorter diameter of the latter, which is surrounded by an oval nebulosity of comparatively very minute proportions. In the principal nebula there is also a minute *core* of greater condensation, north from Merope about one fourth of the longer diameter of the nebula. The outline of Tempel's nebula shows to me as well defined, though of course faint, as the nebula in Orion where it borders on the dark rift near the trapezium."

TRANSIT OF WOLF'S COMET OVER TWO STARS.—The same observer records the transit of Wolf's comet over two small stars on the evening of 1884, Nov. 17. The first star was occulted at $13^h\ 44^m\ 58^s$ G.M.T.: the second about 50^m+ later.

MESSIER.—We are asked by the Rev. S. H. Saxby to invite early attention to a lunar object detected by him on the evening of Feb. 18, in a locality where any variation from the standard authorities is of importance. It is an apparent circular depression, of several miles diameter, at the foot of the outer slope of the Western Messier, on the S.E. side, not to be found either in Schmidt or Neison. This object was so manifest with powers of 130 and 200 upon a $6\frac{1}{8}$ reflector as to almost give a triple character to the Messier group, although a good 100 miles within the terminator.

SUSPECTED VARIABILITY IN TERRESTRIAL LATITUDES.—We learn from Science (Vol. v. No. 102) that, in accordance with a recommendation of the recent Geodetic Conference, a series of observations for latitude is to be made at the U. S. Naval Observatory, which, taken in connection with a similar series made elsewhere, and compared with observations made after an interval of some years, will assist in determining whether there are any slow changes taking place in latitudes upon the Earth. Lisbon, which is very near the same parallel as Washington, is expected to co-operate with the Naval Observatory. The observations will be made with the prime vertical instrument; and at Washington a line officer of the Navy will be detailed for the work, which will probably require several years.

We learn from 'Nature' for Jan. 29, 1885, that the Rev. H. Sumangala, High Priest of Adam's Peak, Ceylon, has contributed to the 'Orientalist' a summary of the views of Hindoo astronomers on the form and attraction of the Earth. Bhāskara, who flourished in the twelfth century, maintained that the Earth, was spherical, and composed of earth, air, water, space, and fire, and that it was surrounded by the Sun, Planets, and Stars. Arguing that the Earth stands firm without support, he says, if there be a material support to the Earth, then something must uphold this support, and so on: ultimately self-support must be assumed, why not assume it in the first instance? Arya Bhatta, in the sixth century, maintained the existence of a diurnal rotation of the Earth round its own axis. The sphere of stars, he states, is stationary, and the earth itself, making a revolution, produces daily rising and setting of stars and planets.

From a letter from Dr. Gould, dated Dec. 18, 1884, addressed to the Editor of the 'Astron. Nachr.,' we learn that at that time he hoped to be able to leave Cordoba by the end of January, or very soon after that date. His address will henceforth be again in the United States; and for the present at Wollaston, Massachusetts.

At a recent meeting of the Government Grant Committee, Prof. Ewing, of Dundee, received a grant of £100 to institute observations of earth-movements on Ben Nevis. He intends looking for minute earth-tremors and also slow movements of the horizon.

Mr. G. W. Hill has published an important paper on Lunar Inequalities due to the Ellipticity of the Earth, which we hope to review at length in our next number.

Approximate Times of Transit of the great Red Spot across the Central Meridian of Jupiter in March 1885.

	h		h		h
Mar. 1....	16·0	Mar. 12....	10·1	Mar. 23....	14·1
2....	11·9	13....	15·9	24....	10·0
3....	7·7	14....	11·8	25....	15·8
4....	13·5	15....	7·6	26....	11·7
5....	9·4	16....	13·4	27....	7·5
6....	15·2	17....	9·3	28....	13·3
7....	11·0	18....	15·1	29....	9·2
8....	6·9	19....	10·9	30....	15·0
9....	12·7	20....	6·8	31....	10·8
10....	8·6	21....	12·6	Apr. 1....	6·7
11....	14·2	22....	8·5	2....	12·5

The red spot was observed here as it passed the central meridian on Feb. 4 at 10^{h} 36^{m} (Long. $147^{\circ}{\cdot}8$) and on Feb. 18 at 12^{h} 4^{m} (Long. $260^{\circ}{\cdot}1$).

When this object began fading the central region of the spot was evidently most affected. This peculiarity has been visible since 1882, until now the spot is reduced to a mere skeleton, the outline only being discernible, so that we have a very similar object in Gledhill's ellipse of 1880–81 (see 'Observatory,' Jan. 1, 1881). At the immediate S. of the spot is a well-defin belt, which shows a deepening tone in that portion occupi same longitude as the spot. With reference to the hollow southern half of the great S. belt, this feature is becomi definite than formerly. The W. shoulder and prolongation belt on this side are growing perceptibly fainter, so that this indicates its approaching obliteration, the manner of it drawn would seem likely to occur in reversed direction to its appearance.

As to the white spot, the predicted times of transit are no this month, owing to some doubt as to its identity. The appeared several fine white spots in the vicinity of the e during the last few months, and it has been practically im to follow them with sufficient closeness owing to lengthy of cloudy weather. The disk of the planet should be fre examined near the times given in Mr. Marth's ephemeris ('M Notices,' Vol. xlv. p. [illegible]), and the positions of any cons white spots visible near the equator carefully estimated r to the passages of the first meridian. W. F. Den

Bristol, Feb. 24, 1885.

Differences of Right Ascension and Declination between Ia and the Centre of Saturn. By A. Marth[*].

Iapetus – Saturn.

Greenwich Noon.	α−A.	δ−D.	Greenwich Noon.	α−A.
	s	″		s
Mar. 1....	−33·9	− 39	Mar. 21....	−13·6
2....	35·1	54	22....	10·8
3....	36·0	68	23....	8·0
4....	36·7	82	24....	5·1
5....	37·1	95	25....	− 2·2
6....	37·3	108	26....	+ 0·7
7....	37·2	120	27....	3·6
8....	36·9	131	28....	6·4
9....	36·3	141	29....	9·2
10....	35·5	150	30....	12·0
11....	34·4	158	31....	14·7
12....	33·1	165	April 1....	17·2
13....	31·7	171	2....	19·7
14....	30·0	175	3....	22·0
15....	28·1	178	4....	24·2
16....	26·0	181	5....	26·3
17....	23·8	182	6....	28·2
18....	21·4	182	7....	29·9
19....	18·9	181	8....	31·5
20....	−16·3	−178	9....	+33·0

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[*] Monthly Notices, Vol. xliv. No. 9.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS (East).

		h
Mar.	1	8·2
	2	6·8
	14	12·8
	15	11·4
	16	10·0
	17	8·6
	18	7·2
	31	11·9
April	1	10·5
	2	9·1
	3	7·7

MIMAS (West).

Mar.	6	12·7
	7	11·3
	8	9·9
	9	8·5
	10	7·1
	22	13·2
	23	11·8
	24	10·4
	25	9·0
	26	7·6
April	8	12·3

HYPERION.

Mar.	14	1·8
April	4	10·9

ENCELADUS.

		h
Mar.	1	4·2
	2	13·1
	3	21·9
	5	6·8
	6	15·7
	8	0·6
	9	9·5
	10	18·4
	12	3·3
	13	12·2
	14	21·1
	16	6·0
	17	14·9
	18	23·8
	20	8·7
	21	17·6
	23	2·5
	24	11·4
	25	20·3
	27	5·2
	28	14·1
	29	23·0
	31	7·8
April	1	16·7
	3	1·6
	4	10·5
	5	19·4
	7	4·3
	8	13·2

TETHYS.

		h
Mar.	1	22·6
	3	20·0
	5	17·3
	7	14·6
	9	11·9
	11	9·2
	13	6·5
	15	3·8
	17	1·1
	18	22·5
	20	19·8
	22	17·1
	24	14·4
	26	11·8
	28	9·1
	30	6·4
April	1	3·7
	3	1·1
	4	22·4
	6	19·7
	8	17·0

TITAN.

Mar.	8	17·8
	24	17·5
April	9	17·6

DIONE.

		h
Mar.	2	13·0
	5	6·7
	8	0·4
	10	18·1
	13	11·8
	16	5·5
	18	23·2
	21	16·9
	24	10·6
	27	4·3
	29	22·1
April	1	15·8
	4	9·5
	7	3·2

RHEA.

Mar.	3	9·6
	7	22·1
	12	10·5
	16	23·0
	21	11·6
	26	0·0
	30	12·6
April	4	1·1
	8	13·6

Apparent Elements of the Ring

Greenwich Noon.	Position-angle of Minor Axis.	Latitude above plane of Ring. Earth.	Sun.
Mar. 19	354° 53′	27° 0′ S.	26° 47′ S.
April 8	354 43	27 3 S.	26 46 S.

Greenwich Noon.	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Semi-maj. axis of Orb. of Titan.
Mar. 19	40″·93	18″·58	27″·22	12″·35	184″·1
April 8	39 ·58	18 ·00	26 ·32	11 ·99	178 ·1

The semi-major axis of the orbit of Titan is given as well as the position-angle of the minor axis of the ring, in order to furnish means for roughly inferring the distances and position-angles of the satellites from the diagram given in Plate I., opposite p. 34.

Maxima and Minima of Variable Stars in March 1885.

M, signifies maximum ; *m*, minimum.

Mar.	2·4	β Lyræ, *m*.
	3	U Canis Min., *m*.
	3·4	T Monocerotis, M.
	3·8	ζ Geminorum, M.
	4	T Geminorum, M.
	6	U Virginis, M.
	8	T Ursæ Majoris, *m*.
	10	T Arietis, *m*.
	14	L² Puppis, M.
	15·4	β Lyræ, *m*.
	16	U Monocerotis, M.
Mar.	16·3	W Virginis, M.
	17	V Coronæ, *m*.
	22	R Coronæ, M.
	22·5	T Monocerotis, *m*.
	25	R Lyræ, *m*.
	25	S Geminorum, M.
	25·3	W Virginis, *m*.
	28	R Vulpeculæ, *m*.
	28·3	β Lyræ, *m*.
	29·2	ζ Geminorum, *m*.
	30·4	T Monocerotis, M.

Variables of Short Period.

δ Cephei.

	d.
Mar.	2·55, M.
	6·25, *m*.
	11·65, *m*.
	13·25, M.
	18·65, M.
	22·35, *m*.
	27·75, *m*.
	29·35, M.

η Aquilæ.

	d.
Mar.	6·7, *m*.
	16·3, M.
	23·4, M.
	30·6, M.

U Sagittarii.

	d.
Mar.	3·4, *m*.
	6·4, M.
	23·7, *m*.
	26·6, M.
	30·4, *m*.

W Sagittarii.

	d.
Mar.	11·4, *m*.
	14·5, M.
	26·5, *m*.
	29·7, M.

U Cephei, *m*.

		h	m
Mar.	19	8	55
	24	8	35
	29	8	14

Algol, *m*.

		h	m
Mar.	1	7	55
	15	15	59
	18	12	48
	21	9	37

λ Tauri, *m*.

		h	m
Mar.	3	14	0
	7	12	53
	11	11	45
	15	10	37

S Cancri, *m*.

		h	m
Mar.	11	15	37
	30	14	53

δ Libræ, *m*.

		h	m
Mar.	1	17	26
	6	9	8
	8	17	0
	13	8	42
	15	16	34
	20	8	16
	22	16	8
	27	7	50

U Coronæ, *m*.

		h	m
Mar.	4	9	41
	11	7	23
	21	15	57
	28	13	40

Astronomical Memoranda, March 1885.

Equation of Time :—Sun *after* Clock, Mar. 1, $12^{m}\ 28^{s}$; Mar. 11, $10^{m}\ 5^{s}$; Mar. 21, $7^{m}\ 12^{s}$; Mar. 31, $4^{m}\ 8^{s}$.

Sidereal Time at Mean Noon :—Mar. 1, $22^{h}\ 37^{m}\ 49^{s}$; Mar. 11, $23^{h}\ 17^{m}\ 15^{s}$; Mar. 21, $23^{h}\ 56^{m}\ 40^{s}$; Mar. 31, $0^{h}\ 36^{m}\ 6^{s}$.

An Annular Eclipse of the Sun, invisible at Greenwich, occurs on March 16. G.M.T. of conjunction in R.A. $6^{h}\ 14^{m}\ 23^{s}{\cdot}7$.

A partial Eclipse of the Moon, magnitude 0·880, occurs on March 30. G.M.T. of middle of Eclipse, $4^{h}\ 34^{m}{\cdot}2$.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk.	
				Lat.	Long.
	h m	h m	° ′	° ′	° ′
Mar. 2	18 42	5 40	337 48	−7 15	267 6
7	18 31	5 49	336 40	7 14	201 14
12	18 20	5 58	335 43	7 11	135 20
17	18 9	6 6	334 55	7 4	69 25
22	17 57	6 14	334 18	6 54	3 30
27	17 46	6 22	333 51	6 40	297 33
32	17 34	6 31	333 35	−6 24	231 35

The Moon.

	rises.			*rises.*			*sets.*			*sets.*	
	h	m		h	m		h	m		h	m
Mar. 1	6	27	Mar. 9	14	45	Mar. 17	7	14	Mar. 25	15	32
2	7	39	10	15	27	18	8	27	26	16	7
3	8	51	11	16	5	19	9	39	27	16	38
4	10	0	12	16	38	20	10	52	28	17	6
5	11	5	13	17	8	21	12	2	29	17	33
6	12	6	14	17	35	22	13	5	30	*rises*	
7	13	4	15	18	1	23	14	2	31	7	40
8	13	58	16	18	27	24	14	51			

Mar. 8d 6h 54m Last Quarter. Mar. 16d 5h 37m New Moon.
23 5 23 First Quarter. 30 4 40 Full Moon.

Planets for February 1885.

Day.	Planet.	R.A.	Dec.	Diam.	Rises.	Tr.	Sets.
		h m s	° ′	″	h m	h m	h m
1	Mercury..	22 14 4	13 15 S.	5·0	18 44	23 39	4 34
11		23 22 7	5 57 S.	5·0	18 32	0 5	5 38
21		0 32 56	3 6 N.	5·2	18 16	0 36	6 56
31		1 40 7	12 1 N.	6·2	17 58	1 4	8 10
1	Venus ..	21 49 49	14 19 S.	10·2	18 24	23 13	4 2
11		22 37 43	10 6 S.	10·0	18 9	23 21	4 33
21		23 24 13	5 26 S.	10·0	17 52	23 28	5 4
31		0 9 51	0 31 S.	9·8	17 32	23 34	5 36
1	Jupiter ..	10 8 30	12 49 N.	41·8	4 18	11 28	18 38
11		10 3 52	13 14 N.	41·4	3 32	10 44	17 56
21		9 59 53	13 35 N.	40·6	2 46	10 1	17 16
31		9 56 50	13 51 N.	39·7	2 3	9 19	16 35
1	Saturn ..	5 5 53	21 38 N.	16·8	22 23	6 27	14 31
31		5 12 14	21 52 N.	16·0	20 30	4 35	12 40
1	Uranus ..	12 7 37	0 1 N.	4·0	7 23	13 27	19 31
31		12 2 56	0 32 N.		5 18	11 25	17 32
1	Neptune .	3 15 19	16 18 N.		21 7	4 37	12 7
31		3 18 11	16 31 N.		19 11	2 42	10 13

Mar. 13d 7h. Mercury in superior conjunction with the Sun.
20 20 Uranus in opposition to the Sun.

Phenomena of Jupiter's Satellites, March 1885.

d	h	m	
2	14	6	iii. Oc. D.
3	16	16	i. Oc. D.
4	13	34	i. Tr. I.
	15	54	i. Tr. E.
5	10	42	i. Oc. D.
	13	20	i. Ec. R.
	16	5	ii. Tr. I.
6	7	19	iii. Tr. E.
	8	0	i. Tr. I.
	10	20	i. Tr. E.
7	7	49	i. Ec. R.
	11	4	ii. Oc. D.
	14	46	ii. Ec. R.
9	8	8	ii. Tr. E.
11	15	19	i. Tr. I.
12	12	27	i. Oc. D.
	15	14	i. Ec. R.
13	7	1	iii. Tr. I.
	9	45	i. Tr. I.
	10	39	iii. Tr. E.
	12	5	i. Tr. E.
14	6	53	i. Oc. D.
	9	43	i. Ec. R.
	13	21	ii. Oc. D.
16	7	28	ii. Tr. I.
	10	24	ii. Tr. E.
19	14	12	i. Oc. D.
20	10	24	iii. Tr. I.
	11	31	i. Tr. I.
	13	51	i. Tr. E.
	14	2	iii. Tr. E.
21	8	38	i. Oc. D.
	11	37	i. Ec. R.
	15	39	ii. Oc. D.
22	8	17	i. Tr. E.
23	9	46	ii. Tr. I.
	12	41	ii. Tr. E.
24	8	49	iv. Oc. D.
	13	29	iv. Oc. R.
25	9	16	ii. Ec. R.
27	13	18	i. Tr. I.
	13	50	iii. Tr. I.
	15	37	i. Tr. E.
28	10	25	i. Oc. D.
	13	32	i. Ec. R.
29	7	44	i. Tr. I.
	10	4	i. Tr. E.
30	8	0	i. Ec. R.
	12	6	ii. Tr. I.
	15	1	ii. Tr. E.
31	10	41	iii. Ec. R.

Occultations, March 1885.

Day.	Star.	Mag.	Disap.	Angle.	Reap.	Angle.
			h m	°	h m	°
6	θ Libræ	4½	12 52	62	14 2	265
9	B.A.C. 6287	6	16 25	115	17 38	243
9	B.A.C. 6292	6	17 6	74	18 26	287
10	ρ' Sagittarii	4	17 18	84	18 38	285
18	B.A.C. 481	6½	6 58	114	7 54	276
21	B.A.C. 1351	6½	6 30	164	6 58	209
21	75 Tauri	6	8 37	71	9 34	295
21	Aldebaran	1	11 43	92	12 32	272
22	111 Tauri	5½	6 52	61	7 54	301
22	117 Tauri	6	9 2	17	9 23	339
24	68 Geminorum	5½	11 39	82	12 37	253
25	B.A.C. 2872	6	12 41	74	13 38	255
26	*h* Leonis	6	13 45	43	14 36	280
27	B.A.C. 3529	6	10 24	30	11 23	284
27	43 Leonis	6	11 37	110	12 28	205
28	B.A.C. 3836	6	10 19	29	11 19	282
28	75 Leonis	5½	12 14	57	13 25	256
28	76 Leonis	6	13 36	14	14 17	301
28	79 Leonis	5½	15 57	73	16 54	249
31	B.A.C. 4591	6	9 20	50	10 26	266

ERRATA IN No. 94.

The accidental omission of "and" on p. 62, line 11, has occasioned a curious misreading. The line should read—"It has also been observed by Prof. Young, and at the Observatory of Algiers." The following errata should also be corrected :—

Page 58, line 5 from foot, *for* 6 *read* 1.
60, line 13 from foot, *for* Mendola *read* Mendota.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 96. APRIL. 1885.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, March 13, 1885.

E. DUNKIN, F.R.S., *President*, in the Chair.

Secretaries: E. B. KNOBEL and
Lieut.-Col. G. L. TUPMAN.

THE Minutes of the Meeting of January 9 were read and confirmed.

Col. Tupman announced that 91 presents had been received since the last meeting. Among them were 3 photographs of stars taken by Mr. Common with his small lens of 4½ in. diameter. One of the photographic plates showed stars down to the ninth magnitude.

General Tennant mentioned that one of the photographs, that of *Altair* and neighbourhood, contained 1800 separate stars which had been identified.

Mr. Lecky. The Fellows will recollect that a few months ago there was a remark made here publicly that the celebrated manufactory of optical crown-glass in Paris was not producing glass as satisfactorily as it used to do. At the beginning of this year I received a circular from M. Feil stating that he had entered into partnership with a gentleman named Mautois, and that M. Charles Feil had returned; and he requested to state here that it was so.

Lord Crawford. I am glad to hear there is a chance of good optical glass being again obtainable. Last time I attempted to procure some it was so bad that whereas I wanted a prism 6 inches square in a face I was not able to get more than 2 inches. Glass seems to have thoroughly gone out of knowledge.

Mr. Lecky. You mean in Paris.

Lord Crawford. Anywhere.

Mr. Lecky. Now that M. Charles Feil has returned to the manufactory there will be every confidence again. We know what he did in old times, and I expect he will do the same now.

General Tennant. A very remarkable example of glass-making is mentioned in the Journal of the Society of Arts by Mr. Siemens. I understand he is able to mould glass at something like 5*s.* or 5*s.* 6*d.* per cwt., of any form you like. Mr. Common has told me that for his 5-inch mirror he paid £300, and that to-day he could get it for £25.

Mr. Hilger mentioned that he had obtained glass 4½ inches square which left nothing to be desired in regard to perfect finishing. There was no polarization, but there was a little deficiency as regards definition.

Mr. Ranyard. Is that Chance's glass?

Mr. Hilger. Yes. I get glass from Paris which is equally good and superior as regards density.

Mr. Downing read a paper on the star places of the 'Nautical Almanac.' In the 'Nautical Almanac' for 1884 a change was made in the source from which the mean places of 182 stars were derived. The places for 1884 were taken from the Greenwich 9-year Catalogue, those for 1883 from the 7-year Catalogues. In this paper he had deduced the systematic discordances resulting from this change in the sources from whence the stars are taken. In looking over the discordances and confining his attention to the R.A.'s of clock stars, as being the most important, $0^s{\cdot}05$ might be considered as an abnormal discrepancy. There were 24 stars with a discrepancy of more than this amount. The average discordance, without regard to sign, for the 24 stars was $0^s{\cdot}064$. These discordances appeared to arise from inaccuracies in the proper motions which had been adopted. These proper motions had been taken from the lists computed by Main and Stone, by comparing Airy's earlier Greenwich observations with Bessel's reduction of Bradley. He had substituted the proper motions deduced by Auwers from comparing the new reduction of Bradley with modern observations, and in every case but two of those stars the discordances had been diminished; and the average discordance was reduced from $0^s{\cdot}064$ to $0^s{\cdot}042$. That appeared to demonstrate the superiority of Auwers's proper motions, and showed the desirability of their adoption in this country. He also remarked that the great majority of large discordances which Auwers had found in his discussion of the 'Nautical Almanac' for 1883 would have disappeared if he had used the 'Nautical Almanac' for 1884.

Mr. Knobel. This is a very important paper, and I think it is extremely satisfactory that Mr. Downing has now shown that the Greenwich 9-year Catalogue adopted in the 'Nautical Almanac' places are so very superior that the change was very desirable. We may be very glad indeed that Mr. Downing has brought this forward. It is very desirable that it should be noticed by the Society because Professor Auwers has pointed out the discordances that exist between the 'Nautical Almanac' for 1883 and the 'Berliner Jahrbuch.'

Mr. Downing read a second paper on the R.A.'s of the Cape

Catalogues for 1850 and 1880. Mr. Gill, in the introduction to the Cape Catalogue for 1850, had made a comparison of the R.A.'s with those of 1880 and found large discordances depending on north-polar distance. He seemed to think that the discordances arose from errors in the right ascensions of the 1880 Catalogue. Mr. Stone, he believed, had expressed the opinion that the discordances arose from the errors in the proper motions used in bringing up the places for some 25 or 30 years. Without wishing to enter into a controversy on the subject, he desired to lay before the Society some evidence which appeared to throw light on the matter. First of all he had compared the right ascensions of the 1850 Catalogue with the 1840 Catalogue, also those of the 1880 Catalogue with the Melbourne Catalogue for 1870, and with the Cape Catalogue for 1860, and by that means he was able to compare the right ascensions both of the 1850 Catalogue and of the 1880 Catalogue with the mean of the three—the Cape Catalogues for 1840 and 1860 and the Melbourne Catalogue for 1870. In the 1880 Catalogue the discordance, depending on north-polar distance, at 120° was $-0^{s}\cdot041$, and at 150° $+0^{s}\cdot030$, being a range of $0^{s}\cdot071$ when compared with the mean of the three. The 1850 Catalogue compared with the mean of the three gave at 100° $-0^{s}\cdot042$, at 140° $+0^{s}\cdot138$, showing that the right ascensions of the 1880 Catalogue were superior to those of the 1850 Catalogue. He had also taken a comparison of each of the Cape Catalogues with the mean of two, viz. the Cape Catalogues for 1840 and 1860. The comparison of the 1880 Catalogue at 120° was $-0^{s}\cdot044$, and at 150° was $+0^{s}\cdot048$, compared with the mean of the Catalogues for 1840 and 1860. The 1850 Catalogue compared with the same mean gave at 100° $-0^{s}\cdot006$, and at 140° $+0^{s}\cdot143$; thus again showing the superiority of the R.A.'s of the Cape Catalogue for 1880. It appeared, therefore, that as far as this evidence went the discordance between the 1850 and 1880 Catalogues was not due to error in the latter.

Mr. Stone. I am glad to find that Mr. Downing has been putting these numbers together. The observations I made at the Cape were made with what I believe to be an efficient instrument, and they were made with extreme care, and were exceedingly good; and I am only too glad to see them subjected to fair tests; at the same time when numbers of this kind are brought forward as real discrepancies, got by comparing the 1880 and 1850 Catalogues, all I can say is that it is a most unreliable test. These observations about 95° and 100° are based on the ordinary clock-star lists of the Greenwich Observatory. Those others at 140° are affected by the whole undetermined proper motions, including the correction to the precession constant between 1850 and 1880; and though people may assume that if you take one portion of the sky there is no tendency to *minus* proper motion, I do not think it is proved at all; and as far as my experience goes, the evidence is against it. There are certain groups of stars where you get a decidedly distinct preponderance of negative proper motions, and therefore it is not at all certain that when you are taking up some 40 or 50 stars about 140° there may not be a preponderance of proper motions in a certain direction. In a test I applied the other day, a fair test, I took the Melbourne observations of about the same period as my own, about 1875, where the case of proper motions scarcely came into play at all; and I showed there was no difference between the Melbourne and Cape observations to any sensible amount. When you are discussing questions of these quantities of $0^s{\cdot}04$ or $0^s{\cdot}05$ you may call them large if you like, but they are only large in a sense, and everybody knows that there are a good many quantities of that kind floating up and down in astronomy. If you adopt a fundamental system of stars, and if you bring them up a certain number of years and adopt them as clock-stars, you get a set of R.A.'s exceedingly good as far as they go; but if you do as is done at Greenwich, refer your stars every few years independently to an epoch, you will get certain right ascensions that would not agree with the others. Which of these methods is the better I cannot say, but I prefer the Greenwich system. If you take the stars from Newcomb's fundamental right ascensions, where he speaks of them, I think, as true to within $0^s{\cdot}01$, and if you compare them with the right ascensions adopted at Greenwich, you will find, for certain R.A.'s, instead of those differences which are supposed to prove the badness of the Cape observations, you have discrepancies three times as large, or twice as large, as much as $0^s{\cdot}08$, and you will find that many of them are of a systematic character; consequently anybody who adopts that system of right ascensions and determines his catalogue from them, and anybody who adopts the Greenwich system, will have a systematic difference. As far as the Cape Catalogue for 1850 is concerned I know as much as most people about it, for it was very nearly complete when I left the Cape, and therefore perhaps I know more than I feel inclined to say on that point. The observations are, I believe, very fair. Only one other point, Mr. Gill seems to lay great stress on the fact that the Transit Circle at the Cape is not a reversible Transit. I should have been exceedingly glad to have had a reversible Transit; but still, after you have got a reversible instrument, how are you going to use it? Are you certain that when you turn the instrument over it is practically the same instrument? Supposing you are going to assume that, I think you will be assuming a thing that no one can assert with any confidence. If you are not going to assume that, you cannot have confidence in the results when you reverse it. If the results agree in the reversed positions, the instrument is good, but reversion unnecessary. But supposing you prove that the observations differ, and differ considerably, then it is possible to say the mean is nearest the truth; but there is no means of proving it.

Mr. Knobel read a paper by *Professor Oppolzer* on the proposed change in the astronomical day, advocating the adoption of universal time, reckoning from Greenwich midnight, and pointing out that little difficulty would be experienced in making the necessary changes consequent on the change in the method of reckoning time.

The Astronomer Royal. With reference to this paper of Professor Oppolzer's, I may say I have since received a letter from him in which he says he is going to give practical effect to his views by adopting the new reckoning of time in a publication he is now preparing, comprising a list of 8000 solar and 5200 lunar eclipses between the years −1207 and +2161, so that will give an opportunity of seeing whether all the inconveniences resulting from the change are really so great as some astronomers suggest. I must say I agree with his opinion that we are inclined to overestimate the inconvenience and trouble caused by a change of this sort, rather than underestimate it. I have been rather surprised, myself, at the ease with which the change has been made at Greenwich. We have practically adopted the new reckoning for all uses there, without any trouble; and for certain purposes we keep up the old reckoning for comparison with the 'Nautical Almanac,' and the change is so easy that we can at any time adopt the old or the new in the published observations. I think when the change can be so easily made as that, it is not a matter of such serious inconvenience as we have been led to infer. I am also glad to say I have received a letter from M. Struve, Director of the Pulkowa Observatory, saying that he intended to adopt the new reckoning of time there for internal use. The only thing he thinks requires consideration is the epoch at which the change should be introduced into the published observations, and he is inclined to recommend that that should be deferred till the new time can be adopted in the 'Nautical Almanac.' That would be in the year 1890, the first year in which it can be conveniently adopted for that purpose. I think it is very satisfactory to find that Professor Oppolzer has expressed his opinion so strongly, because at the Washington Conference there was no scientific delegate from Austria present, and the Austrian delegate present abstained from voting. Prof. Oppolzer speaks with authority as the scientific representative of Austria. I think this paper is therefore of very great importance, as bearing on the question.

Mr. Stone. The idea of Professor Struve is that some convenient time for making the change should be fixed, and I think that is essential. But we must recollect that those who come after us will have, in all comparisons between past and future observations, to make an allowance for this loss of 12 hours, and therefore the period at which the change is made must be marked in the most emphatic manner in our power. It will certainly cause a great deal of inconvenience to our successors, and I think we should reduce this inconvenience to a minimum. There is one point which, as there has been a good deal of discussion at these meetings upon some questions I have raised here, I may be allowed to call attention to, and that is that we must give our sidereal times, in future, not for 0^h, but for our new 0^h, being the 12 o'clock preceding. Of course that can be very easily done, but if any one attempts to do it he will find that the Sun's position between the old 0^h and the new 0^h will have changed by about two minutes of right ascension. Therefore if any one made a change in the same way as we have made changes since 1863, by simply altering the Sun's position without introducing a corresponding change in sidereal time, they would find all their times changed by two minutes, whereas we shall have made an alteration of 12 hours.

General Tennant. I am fully with my friend Mr. Stone in his statement; but I want to point out the real difficulty seems to me to come to this, that we must never forget that Greenwich mean time is a phrase in the English language which means something definite, and we must consider it to have its origin at midnight, and we must call the day that begins at midnight universal time. If we do not take a new name for it, we shall have endless confusion. If we persevere we shall very soon find things will settle themselves down.

Mr. Stone. But it is not desirable they should settle themselves down until a point of that kind is settled. Individually, if I might be allowed to say so, expressing an opinion as an astronomer, I should say for astronomy there is only one natural unit of time, and that is sidereal time. That I thoroughly go with, and I intend shortly to bring forward a small treatise of my own, showing I have worked out on my own lines the points connected with it.

The Astronomer Royal. I quite agree with Mr. Stone that the natural unit of astronomy is the sidereal day; but unfortunately the natural unit of astronomers, in common with other people, is the solar day, because they have to regulate their daily life by it.

Capt. Wharton. I wish to remind the Society that there is another class of people who are very considerably affected by this, besides astronomers, and that is sailors. If any change is made in the time, it will, unless it is done after a very considerable amount of notice being given, and caution, cause disaster, because although an educated sailor will have no difficulty in adding his twelve hours on or taking them off, there are a great many ships, I am sorry to say, belonging to the English nation, which are navigated by rule of thumb. I simply meant to warn the Society that there are other people to remember besides astronomers, and they are the people whom the Governments will have to consider. I am a sailor myself, and may speak with a bias, but I think this is the view the Governments of the world will take in making any formal change, and I think it should be done formally. It has been initiated by a formal conference of delegates from all nations of the world, and I do not think it is for any Society or any individual to attempt to push the thing forward before the time is ripe. I am in favour of the change myself. If you can get one day of 24 hours instead of two distinct days, it will be a great improvement; but my own opinion is, considering that a great many of the delegates at Washington abstained from voting on some of these points, that it will merely tend to throw things back if one Society in the nation that will gain most by it attempts to ram it down the throats of foreigners who are opposed to it. I myself would counsel a little delay.

General Strachey. My having been present as one of the delegates of Great Britain at Washington, perhaps, entitles me to say a word as to the nature of the conclusions that were come to there. I would say, with reference to the remarks which fell from the Hydrographer, that the assemblage at Washington was not a scientific conference; it was a diplomatic conference. There were, however, of course, some eminent scientific members of the conference; for instance, we had the advantage of the presence of Professor Adams. The bulk of the delegates went to Washington with instructions from their Governments how to vote, and the instructions, as a rule, were "Vote for what was settled at Rome." A representative of one of the most important states of Europe said to me, "I have plenary powers to vote as I please, subject to the condition that I am to vote for Greenwich Meridian; anything that follows on Greenwich Meridian as regards time I will accept." He, however, abstained from voting. I entirely agree with Prof. Oppolzer's excellent letter, and with the views of the Astronomer Royal, and obviously it is quite certain if any change is made it must be made with great caution, and it can only be made, as I think, after further consideration of what is the intention of the States who joined the conference at Washington. It must be remembered that our Government, when it sent a representative to Washington, virtually accepted the position that no change would be made in this matter of longitude, or of time, without consulting the rest of what I may call the civilized world. The Conference at Washington having come to this conclusion, it remains with the Government of the United States to take further action in the matter, and up to the present time they have not taken action. It so happened that at the time the Conference was sitting the change in the Presidency of the United States took place, and the Government has not taken any action on the report of the Conference; but before very long we may expect that the United States Government will take action in the matter, and it will rest with our Government to move; but it would not be decent for us, having pledged ourselves to consult with the rest of the civilized world, to take independent action. But no doubt that reserve on our part should not last beyond a reasonable length of time. If it is the view that a reasonable time has elapsed, that the other nations of the earth do not intend to accept the conclusions that were come to at Washington, then I look upon it that it is perfectly open to English astronomers, and any other astronomers in the world that chose to join with them, to act in a manner which they think is most conducive to the convenience of the world. I confess I have very little doubt that in one way or another, sooner or later, the meridian of Greenwich will be accepted, and the civil day of Greenwich will be accepted as the practical working day—I will not say for astronomy in the sense Mr. Stone has used it, but in the ordinary use to which it was proposed that the universal day should be applied.

The President. I am sure you will return your thanks to Prof Oppolzer for his paper, and, if for nothing else, for the very interesting discussion we have had on the subject. It is very gratifying that we have had General Strachey present with us to-night, and that he has favoured us with some very interesting remarks.

Mr. Spitta described a Note he had presented in reference to the Transit of the fourth satellite of Jupiter. It was black in colour and oval in shape, and its long axis was directed parallel to the belts.

Mr. Ranyard. Was it the shadow or the satellite?

Mr. Spitta. The satellite itself.

Mr. Knobel. I observed the entrance of this satellite on the occasion Mr. Spitta mentions, but it was between intervals of clouds, and I certainly remarked one point that was curious. When first seen, after getting within the disk, it was a bright spot, five minutes after a very small dark spot, and in about a quarter of an hour it seemed a perfectly round black spot.

Mr. Spitta. I did not observe at ingress at all.

Mr. Knobel. I was watching the transit of the shadow of the first satellite two hours before, and was particularly struck how round the fourth satellite appeared to be.

Mr. Spitta. It always appeared to me before to be a round spot.

Mr. Common. What was the observation made with?

Mr. Spitta. It was made with a 10-inch reflector.

Mr. Common. The reflector is an instrument that is rather subject to changes of errors in adjustment, and it has a tendency to make the object elliptical.

Mr. Spitta. I have tested the adjustments, and have found nothing wrong.

Mr. Common. Was Mr. Knobel's instrument also a reflector?

Mr. Knobel. Yes.

Mr. Common. If you found it elongated the other way it would be difficult to explain it. Mr. Knobel found it round; Mr. Spitta found it elliptical. Especially with a reflector the greatest caution is necessary in the adjustment.

Mr. Spitta. I was observing with a refractor at the same time.

Mr. Common. And did you see it?

Mr. Spitta. No, not distinctly.

Mr. Common. I should not think such a slight observation as that would be sufficient to justify the statement that the satellite was elliptical.

Mr. Spitta. I said it "appeared to be." I was cautious in the statement.

Lord Crawford. I was in hopes you would have had a note from my observatory at Dunecht on the subject of the phenomena observed there as to one of Jupiter's satellites. The observation was made by Dr. Copeland of the occultation by the first satellite of Jupiter of its own shadow, which is a rare phenomenon. There was no time to print a circular, but they telegraphed to six or seven observatories to look out for it; but I do not know what result has come from it.

Mr. Knobel read a note by *Capt. Noble* on the 'Nautical Almanac' dimensions of the Saturnian system, stating that the ball of the planet was smaller in the Almanac than appeared in the telescope.

Mr. Green. I noticed a discrepancy last year very marked indeed, and I have been trying again this year, and was obliged to make a slight allowance in the 'Nautical Almanac' proportions in order to bring the ball up to Cassini's division.

Mr. Stone. I do not see Mr. Hind here; but I think, when the matter was brought before the Council, he said they were making some changes in the elements used in the 'Nautical Almanac,' so that I think they are aware that the theoretical elements are not perfect.

The Astronomer Royal read a paper on the spectroscopic observations of the motions of stars in line of sight in the year 1884 at Greenwich. These observations were made by Mr. Maunder and Mr. Nash with the spectroscope, and carried on in the usual way. There are one or two points of interest which deserve mention. An unusually large number of stars, namely 54 stars, have been observed, each on an average of six nights, in the course of the year 1884. Besides that, observations have been made of the spectra of the Moon, the sky, Venus, Mars, and the limbs of Jupiter as a check on the general trustworthiness of the results for stars. The displacement in the case of Venus is somewhat larger than the computed value; that for Mars is not so far out. They are both in the right direction. The two limbs of Jupiter show displacements which agree very well with the computed value. Of course it must be understood those measures are subject to very large casual errors of observation, which may fall either one way or the other; and it is hoped that, by continuing them carefully, sufficient material may be accumulated to determine mean results which will be fairly trustworthy. There is one particular case, that of Sirius, which should be specially referred to. Mr. Maunder has observed that star on 13 nights, and made 45 measures, giving the mean motion of approach as 22 miles per second, which is in the same direction noticed of late years, opposite to that which has been observed by Dr. Huggins and in the earlier Greenwich results; so that now we have got a progressive change of motion from 25 miles per second of recession to 20 miles or so of approach—a very extraordinary thing, and no satisfactory explanation of it can be offered at present. A word may be added about this. Special pains have been taken to make these observations of Sirius at different times of the year, because in determining the motion of stars in line of sight by the spectroscope it is conceivable there might be a very rapid motion of comparatively short period. There might be a motion of revolution round of a body close to Sirius in the course of a few months, which would be quite inappreciable by the ordinary method of observation, but which could be detected by the spectroscope. Therefore observations should be made throughout the year, as far as may be. They have been made at Greenwich in the months of January, February, March, April, and December, and the results hitherto are fairly accordant, so that it is evident that there is no such motion of short period.

Mr. Maunder. There is a point which may be of interest that has been suggested to me by some of my recent observations with the spectroscope; and that is, that the character of the lines of one or two stars seems to have undergone some slight change. For instance, during the years of 1875-6-7 I found the F line in the spectrum of Altair very difficult to observe, but latterly the measures have been much more accordant, and I have felt much more satisfied with them; and, again, there were one or two stars the lines in which I had very little difficulty in observing some years ago, which seem decidedly more difficult now. Something of the same sort has been suggested with regard to the lines of the solar spectrum. Mr. Lockyer pointed out some years ago that there was evidence of the magnesium lines changing from the time of maximum activity to that of minimum; and undoubtedly the lines in spot-spectra do change their character at different times of the Sun's activity, so that it is possible that similar changes in the character of the lines may also occur in the spectra of some of the stars. I can hardly say that our observations yet are sufficient to do more than suggest the possibility of such a change; but at any rate I thought I would mention my suspicion.

Mr. Ranyard. The stellar spectrum is an integration spectrum from the whole surface of a star. It would be a much more wonderful change than a change that took place over any limited area. When one looks at the star-spectrum all the light is massed together from all parts.

Mr. Maunder. Mr. Lockyer was referring to the *general* spectrum of the Sun. You will find his suggestion in his 'Solar Physics.' The idea is based upon drawings, not upon photographs. Photographic records would be more satisfactory, of course, as a basis for such a suggestion.

Mr. Ranyard. Changes of brightness might have occurred; but I do not know that there is any definite evidence as to whether there is any change of relative intensity of the lines in the sun-spot period.

Mr. Downing. As to the case of Sirius, it is not favourably situated for observation at Greenwich, on account of its great zenith-distance. It is desirable observations should be made at some observatory where the star attains a greater altitude—some observatory in the south of Europe (Nice, for instance) would be best.

Lord Crawford. I should like to know if there is any statement to be made by the Council with reference to the matter of a circular I signed as to the question of proxy voting.

After a discussion as to whether a matter referring to a change in the Bye-laws should be brought before an Ordinary Meeting of the Society, the following Resolution of the Council on the subject mentioned by Lord Crawford was read by *the President*:—"That the signatories to the request to the Council to take steps for altering the Bye-laws so as to permit Fellows unable to be present at the Annual Meeting to vote by proxy be informed that the Council do not propose to recommend such a change; the Council, however, are of opinion that a proposal to enable all Fellows who cannot attend the Annual General Meeting to vote for the election of the Officers and Council by post, under suitable precautions, might fitly form a subject for discussion at a Special General Meeting." *The President* continued: That is the special resolution which has been, not unanimously, but very nearly unanimously, passed by the Council.

The following papers were announced:—

Rev. S. J. Perry. "Phenomena of Jupiter's Satellites, and Occultations of Stars by the Moon, observed at Stonyhurst, 1884."

A. S. Williams. "Account of some Physical Observations of Venus in 1884."

Rev. J. Pearson. "Solar Eclipses and Occultations by the Moon."

E. von Gothard. "On Variations in the Light of β Lyræ."

E. Neison. "Observations of Barnard's Comet (*b* 1884)."

E. Neison. "On the Value of the Long Inequality in the Motion of the Moon due to the disturbing action of the planet Mars."

H. Sadler. "Notes and Corrections to Sir J. Herschel's Synopsis of all Sir W. Herschel's Micrometrical Measurements."

E. J. Spitta. "Jupiter's Fourth Satellite."

A. Marth. Data for a Graphical Representation of the Solar System." (Continuation.)

The following gentlemen were elected Fellows of the Society:—Prof. W. Steadman Aldis, M.A., Lieut. H. E. O'Neil, and W. Peck.

The meeting adjourned at 10 o'clock P.M.

Photometric Observations of Neptune at the Harvard College Observatory.

THE observation of Neptune with the meridian photometer of this Observatory has been continued in the manner described in a former communication ('The Observatory,' vii. p. 134). The results are given below, in the form previously employed. The successive columns contain the numbers of the series, the date, the Greenwich mean time, the magnitude of Neptune, the residual from the mean 7·6, and the initial of the observer.

No. of Series.	Date. 1884–5.	G.M.T. h	Magn.	Resid.	Obs.
372	Dec. 16	14·3	7·8	+0·2	W.
373	Dec. 19	14·1	7·6	0·0	P.
375	Dec. 20	14·0	7·5	−0·1	P.
377	Dec. 30	13·4	7·5	−0·1	W.
378	Jan. 2	13·2	7·5	−0·1	P.
380	Jan. 10	12·6	7·5	−0·1	P.
383	Jan. 14	12·4	7·7	+0·1	P.
385	Jan. 17	12·2	7·6	0·0	P.
388	Jan. 21	11·9	7·6	0·0	P.

The mean result, carried to hundredths, is 7·59, which requires an additional correction of +0·04 for the combined effect of atmosphere absorption, instrumental error, and reduction to mean opposition. The corrected result is accordingly 7·63.

Upon comparing the observations with those printed in the former communications, and taking the residuals from the general mean 7·7, we find one residual of +0·2, January 12, 1883, and four residuals of −0·2, December 20 and 30, 1884, and January 2 and 10, 1885. These are the greatest discrepancies, and are within the limits of accidental error in the observations. The results for three seasons are 7·71, 7·77, and 7·63, with corresponding average deviations of ·09, ·08, and ·08.

It has been remarked by Mr. Hall ('Monthly Notices of the R. Astr. Society,' xliv. p. 260) that the period of variation resulting from his observations of Neptune is such, that the variation cannot be detected by means of observation at nearly the same hour on successive nights. The accordance of the results obtained in three successive seasons, however, makes it improbable that there is any variation in the light of Neptune of a strictly periodic character. Mr. Hall, indeed, regards the changes which he noticed as the result of temporary disturbances at the surface of the planet.

In the consideration of apparent cases of variation in periods, two or three of which are nearly equivalent to one day, the influence of relative position upon the observed brightness of the objects compared must be borne in mind. This influence has frequently been noticed, but is often too much neglected by observers. It has been discussed in the Annals of this Observatory, xi. p. 222, and xiv. p. 41. In observing an object east and west of the meridian during the same night, the effect of relative position must often produce an appearance of variation. This consideration may perhaps account to some extent for the variations of Neptune occurring in Mr. Hall's observations.

It is worth notice that a similar remark may perhaps apply to the variations of U Cephei. The period of this star is so nearly

2½ sidereal days, that the alternate minima are observed at a given station, in hour-angles differing by very nearly 180°, with the comparison stars in reversed positions with respect to the variable. Further observation will therefore be required to confirm the difference reported by Mr. Knott in the character of alternate minima.
E. C. Pickering.

Harvard College Observatory,
Cambridge, Mass., 1885, Feb. 13.

Variations of Latitude[*].

The International Geodetic Conference held at Rome in October 1883, presented to the notice of astronomers the question of the variability of latitudes. A plan of observation was proposed to the Conference by Mr. Fergola, an Italian astronomer, who has made the matter a special study, and this plan was referred to a committee composed of Messrs. Villarceau, Bakhuyzen, Cutts, and Schiaparelli. This committee reported favourably, and in due time the proposed plan has come to the Superintendent of the Naval Observatory through the veteran leader of geodesy, General Bäyer, the head of the Prussian Geodetic Survey.

According to the plan of Mr. Fergola, the variations of latitude are to be investigated by special series of observations, made with the best prime vertical transit instruments on selected lists of stars. An important feature of the plan is that the work is to be chiefly differential. Two observatories are to co-operate—for example, one in Europe and one in America; and these stations are chosen so that the difference of latitude is small. It being thought best to confine the observations to established observatories, in order that similar series may be more certainly made in the future, Mr. Fergola has selected the following stations:—

Stations selected.	Diff. lat.	Diff. long.
Cape of Good Hope and Sydney ……	4′ 22″	8^h 51^m
Santiago and Windsor (N.S.W.) …..	9 47	9 14
Rome and Chicago……………….	3 53	6 40
Naples and New York (Columbia Coll.).	6 22	5 53
Lisbon and Washington …………	11 7	4 31

At such stations as these the stars that pass near the zenith, and which are best suited for determinations of latitude, can be observed at both stations at nearly the same zenith distance. The conditions would be such that the variations of refraction would be small; and if the observations are made with care and skill, and with the best appliances of modern astronomy, the difference of latitude of the two stations must be determined with the utmost accuracy. If the observations should be repeated after an

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[*] From the 'American Journal of Science' for March 1885

Observer.	Date.	Latitude.
Peters..........	1843	+59° 46′ 18″·73±0″·013
Gyldèn	1866	18 ·65±0 ·014
Nyrèn..........	1872	18 ·50±0 ·014

Here again we notice a small diminution of latitude. In each determination also the observations are so numerous that probably the accidental errors of the observer have been almost completely eliminated. Thus the number of Peters's determinations is 371, Gyldèn's 236, and Nyrèn's 155; all from observations of the pole star. It may seem hypercritical to doubt results that have been deduced with so much care and skill, both in observing and reducing; but still we must remember that these are absolute determinations of latitude, that three different observers took part in the work, and that the variation from Peters to Nyrèn is only 0″·23. An astronomer of experience would be cautious, I think, in ascribing this difference to an actual change of latitude. But the evidence in this case supports that drawn from Mr. Fergola's table. It is well, therefore, to examine how far more recent determinations confirm the results of this table. A recent and very complete discussion of the latitude of the Greenwich Observatory by Mr. Christie, the present Astronomer Royal of England, gives, from the observations of the years 1836 to 1879, the following values of the latitude:—

1836–1849. Latitude = 51° 28′ 38″·15
1851–1865. Latitude = 38 ·13
1866–1879. Latitude = 38 ·17

These results give no evidence of a secular change of latitude. Again, a determination of the latitude of Washington, in 1883, gives Latitude = 38° 53′ 38″·94, and this result also furnishes no proof of a change of latitude. These negative results show that in the case of the latitude of Pulkowa we must wait for further proof before assuming a real secular change in the latitude of that Observatory.

So far as periodical changes of latitude are concerned, there is a theoretical reason why such changes might occur. If our Earth at the origin of things received its motion around the Sun and its motion of rotation on an axis from a single impulse, the direction of the impulse would pass about twenty miles from the Earth's centre, and it would seem to be but little probable that the Earth would begin to rotate around one of its principal axes. But the observed constancy of latitude shows that such was nearly the case, or that some cause has acted to bring the instantaneous axis of rotation practically into coincidence with the principal axis. These periodical changes depend on the structure of the Earth and the distribution of its matter. They are, therefore, quite different from precession and nutation, which are produced by exterior disturbing forces, chiefly by the action of the Sun and Moon.

[illegible] the question of the variab [illegible] subjected to a severe test. [illegible] raised an old question which wa [illegible] among astronomers. Two centuries a [illegible] during the year was a common or [illegible] and the complete reduction of o [illegible] tables of refraction, have caused the disapp [illegible] of the anomalies that formerly appeared, and the [illegible] tended toward the idea that latitudes have no s [illegible] this is a result that [illegible] would not [illegible] probable. Thus we might have geological change would alter the position of the vertical line at a station: or such as would cause a gradual or secular change in the pos the earth's axis of rotation, by shifting large masses of ma the earth's surface or in its interior, and these changes produce variations of latitude. In fact, it is for the pur testing such changes that Mr. Fergola has chosen stations have a large difference of longitude.

The table of latitudes published some years ago by this nomer does seem to show a secular change in the latit northern observatories. This table is as follows:—

Place.	Date.	Latitude.
Washington ..	1845	+38° 53 39 ·25
Washington	1863	38 ·78
Paris	1825	48 50 13 ·0
Paris	1853	11 ·2
Milan	1811	45 27 60 ·7
Milan	1871	59 ·19
Rome	1810	41 53 54 ·26
Rome	1866	54 ·09
Naples	1820	40 51 46 ·63
Naples	1871	45 ·41
Königsberg	1820	54 42 50 ·71
Königsberg	1843	50 ·56
Greenwich	1838	51 28 38 ·43
Greenwich	1845	38 ·17
Greenwich	1856	37 ·92

It will be seen here that without a single exception the la given in the table have diminished during the present c But these variations are small, and during these intervals c so many changes have occurred in the instruments, and the astronomers, that the diminution of latitude may b apparent. The most complete investigation of this ques that by Mr. Nyrèn of the Pulkowa Observatory. In this c observations have all been made with the same instrumen the observations are of remarkable accuracy. The probabl of a single determination of latitude is only ±0″·2. The for the latitude of Pulkowa are:—

Observer.	Date.	Latitude.
Peters	1843	+59° 46′ 18″·73 ± 0″·013
Gyldèn	1866	18 ·65 ± 0 ·014
Nyrèn	1872	18 ·50 ± 0 ·014

Here again we notice a small diminution of latitude. In each determination also the observations are so numerous that probably the accidental errors of the observer have been almost completely eliminated. Thus the number of Peters's determinations is 371, Gyldèn's 236, and Nyrèn's 155; all from observations of the pole star. It may seem hypercritical to doubt results that have been deduced with so much care and skill, both in observing and reducing; but still we must remember that these are absolute determinations of latitude, that three different observers took part in the work, and that the variation from Peters to Nyrèn is only 0″·23. An astronomer of experience would be cautious, I think, in ascribing this difference to an actual change of latitude. But the evidence in this case supports that drawn from Mr. Fergola's table. It is well, therefore, to examine how far more recent determinations confirm the results of this table. A recent and very complete discussion of the latitude of the Greenwich Observatory by Mr. Christie, the present Astronomer Royal of England, gives, from the observations of the years 1836 to 1879, the following values of the latitude:—

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So far as periodical changes of latitude are concerned, there is a theoretical reason why such changes might occur. If our Earth at the origin of things received its motion around the Sun and its motion of rotation on an axis from a single impulse, the direction of the impulse would pass about twenty miles from the Earth's centre, and it would seem to be but little probable that the Earth would begin to rotate around one of its principal axes. But the observed constancy of latitude shows that such was nearly the case, or that some cause has acted to bring the instantaneous axis of rotation practically into coincidence with the principal axis. These periodical changes depend on the structure of the Earth and the distribution of its matter. They are, therefore, quite different from precession and nutation, which are produced by exterior disturbing forces, chiefly by the action of the Sun and Moon.

The theory of these periodic changes was given by Euler in 1765. The period of the change of latitude depends on the moments of inertia of the Earth, and the values of these moments are given approximately from the observed values of the precession and nutation. An expression of the periodical change of latitude is found easily from Euler's equations for the motion of a rigid body. This results from the first step in the approximate solution of these equations, but it is sufficient for all purposes of observations. If A, B, C are the moments of inertia about the principal axes, and ϕ denotes the latitude, the general expression for the latitude is

$$\phi = \phi_0 + \rho \, . \cos \left\{ \sqrt{\frac{(C-A)\,(C-B)}{A\,B}}\, n \, . \, t + \xi \right\},$$

in which $\phi_0 + \rho \cos \xi$ is the latitude at the epoch of t, n is the velocity of rotation of the Earth on its axis, and ρ and ξ are constants to be found from observations. The expression under the radical sign must be positive, since otherwise the motion of rotation of the Earth would not be stable. If we substitute the values of the moments of inertia, the expression takes the following form, in which the unit of time is a year:

$$\phi = \phi_0 + \rho \, . \cos \, (431^{\circ}\!\cdot\!0 \, . \, t + \xi).$$

Hence the instantaneous axis of rotation makes a revolution around the principal axis in 305 days.

We have now to see what the observations have shown in regard to this periodical variation of latitude. The first careful investigation of this question was made by C. A. F. Peters, of Pulkowa, from his excellent series of observations with the Ertel vertical circle. These observations extend over thirteen months, and from their discussion Peters found for 1842, $\rho = 0''\!\cdot\!079 \pm 0''\!\cdot\!017$. A similar discussion was made by Nyrèn of the three series of observations made by Peters, Gyldèn, and himself with the same instrument, and the results are:—

$\rho = 0''\!\cdot\!101 \pm 0''\!\cdot\!014$, Peters, 1842.
$\rho = 0\;\cdot\!125 \pm 0\;\cdot\!020$, Gyldèn, 1866.
$\rho = 0\;\cdot\!058 \pm 0\;\cdot\!015$, Nyrèn, 1872.

This is a very good agreement of the results, and it is worth while to notice that the parallax of the pole star comes out positive from each series, and that the resulting value of the constant of aberration agrees well with the best determinations. An investigation made by Mr. A. M. W. Downing of the latitude of Greenwich from the observations for the ten years 1868–1877 gives $\rho = 0''\!\cdot\!075 \pm 0''\!\cdot\!015$. The first thing we notice about these results is the smallness of ρ, which is the maximum variation of latitude, and also the fact that in every case it is from four to eight times as great as its probable error. From this relation of the error one might infer the reality of ρ, but we ought to consider the form of the equation of condition.

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the one limb ought to be slightly displaced relatively to the same line in the spectrum of the other. This has been observed to be the case, and the period of rotation of the Sun as deduced by the present Astronomer Royal, by Prof. Young, and other observers, from measures of the amount of this displacement, agrees very fairly well with that derived from observations of Sun-spots.

But this displacement has been exhibited in a much more striking manner. Many of the lines in the solar spectrum are due to the action of our own atmosphere, and as this of course does not partake in the solar rotation these lines do not suffer displacement at the limbs. Prof. Hastings, and following him M. Thollon, brings the images of the east and west limbs into juxtaposition on the slit, and is able at once to discriminate between the lines of solar and those of telluric origin; the former not corresponding in the two spectra, the latter coinciding throughout. M. Cornu has developed the same idea in a very pretty and ingenious manner, and by making the image of the Sun oscillate across the slit, he makes the solar lines seem to oscillate also, whilst the telluric lines remain unmoved.

The planet Jupiter, from the wonderful velocity with which it rotates, affords another means of demonstrating this displacement to the eye. The difference of motion of the two limbs is more than 15 miles per second, and the effect of this difference is doubled by the fact that Jupiter shines by reflected, not inherent light. The relative displacement therefore is equivalent to that produced by a motion of 30 miles per second, a very appreciable amount. If the image of the planet be made to move rapidly to and fro across the slit, the slit being placed parallel to the axis of the planet, the lines of the spectrum will show an obvious oscillation backwards and forwards to correspond; and that this is no fictitious effect can be ascertained by turning the spectroscope until the slit is parallel to the equator, when no such effect can be produced. With the slit on the equator of the planet the lines of the spectrum are inclined, and their extremities bend different ways; with the slit at right angles to the equator and on the central meridian, the lines are upright and straight.

There is therefore no doubt as to the truth of the theory. If a heavenly body is approaching us at a sufficiently high speed, the lines of its spectrum will suffer an observable displacement towards the violet; if it be receding from us, the displacement will be towards the red. But the actual measurement of the amount of this displacement is, in the case of a star, not altogether an easy matter. There is, first, the smallness of the amount to be measured. A spectroscope which will not clearly separate between the two lines of the close pair at λ 4890 will certainly be inefficient for this purpose. Angström gives these lines as being 0·79 tenth-metres apart, which would correspond to a motion of 32 miles per second. Before, therefore, we can express the motion of a star in miles per second, and attribute any real meaning to the figure in

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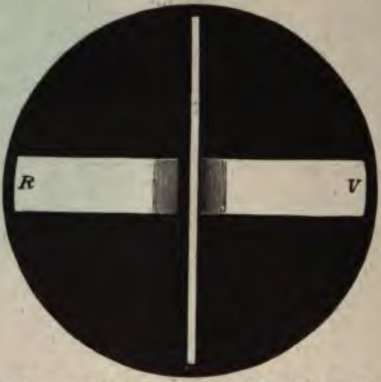
nearly always employed, and the object-glass being corrected so as to bring together these rays and the yet more brilliant part of the spectrum towards the yellow, the visual focus is practically correct; but if a line in the violet were used, the difference between the two foci would be too great to be neglected. Lastly, an image of the vacuum-tube or spark is formed by a small condensing-lens and brought to focus on the slit. In the Greenwich spectroscope the vacuum-tube is to the left of the slit, and nearly in the same plane as the face of the slit plate, the rays being reflected into the spectroscope by means of a small piece of unsilvered plate glass, placed before the slit and making an angle of 45° with the slit plate. A better arrangement, in some respects, would be to fix the spark-holder either above or below the slit, and the reflector with its edge perpendicular to the slit instead of parallel to it. The light from the star passes through the plane glass reflector and the two spectra are therefore superposed, as seen in fig. 1, which represents the relative positions and appearances of the F line in Sirius, and the hydrogen line Hβ given by a vacuum-tube as observed in December 1875. This arrangement is found to answer better than the employment of total reflection prisms, since with these the two spectra can only be seen side by side, and fresh possibilities of error in measurement are introduced. The loss of light from the vacuum-tube or spark by the reflection from the unsilvered glass is a positive advantage, as otherwise the comparison spectrum is so brilliant as to "kill" the spectrum of the star.

Fig. 1.



F line in the Spectrum of Sirius as compared with Hβ line of Hydrogen, 1875 December.

These points having been duly attended to, the telescope may be turned to some bright star. A cylindrical lens is then placed in front of the slit and at such a distance from it as to give a conve-

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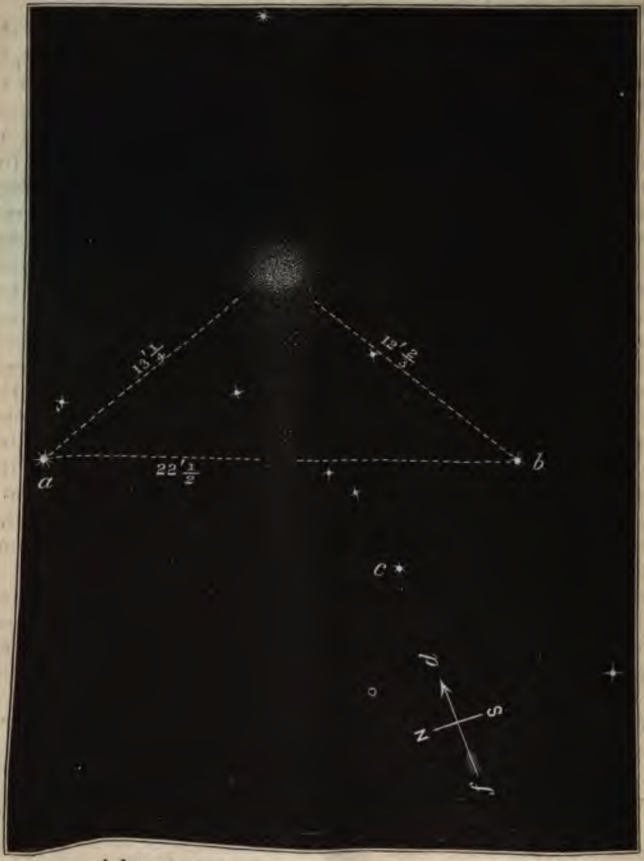
carded, and the tip of the pointer is rendered visible by allowing to project some little way on the spectrum of the star. Tl practically lessens the breadth of the spectrum available for obse vation, but the drawback is a smaller one than those accompanyi most of the methods of illumination. E. W. MAUNDER.

[To be continued.]

Astronomical Notes.

ENCKE'S COMET.—This comet has been observed here frequent since January 2, mostly with the 5-inch telescope, while come seeking.



On the night of February 11 a faint tail was noticed at abou 7 P.M.; this extended some 10' in a direction slightly *n.f.* Tl tail pointed directly towards and nearly reached an 8th mag. star in the same field with the comet.

On the night of February 13 a careful examination of the comet was made with the six-inch Cook Equatoreal of this Observatory, and the enclosed sketch secured. The comet was small and brightly condensed; the point of greatest brightness was not central, but very slightly following the centre in the direction of the tail. The tail was very slender, faint, and straight, and extended about 14′ or 15′, by estimation, in a direction about 20° north of following.

The sketch was made at $7^h\ 13^m$ Nashville mean time, a power of about 90 being used. The star *a* was the brightest of several in the field, probably of the 8th or 9th magnitude. The direction of sidereal motion passed through the comet and star *c*. The distances were estimated with the aid of a ring in the eyepiece.

On February 16 the tail to Encke's Comet was a little longer and slightly more distinct than on the 13th. The general appearance of the comet was the same as on the 13th.

NEW NEBULÆ.—The following new nebulæ (besides seven others) have been found whilst engaged in comet-seeking with my 5-inch refractor.

	Right Ascension, 1885·0.			Declination, 1885·0.		
	h	m	s	°	′	″
a........	3	14	0·0	−19	30	43
b........	3	14	54·6	−26	28	43
c........	8	13	59·3	−36	44	57
d........	9	12	42·0	−21	52	11

(*a*) Small, round, and very much brighter, somewhat suddenly, in the centre. Rather faint from its generally low altitude. It is south following a 9th mag. star by $1\frac{1}{4}'$.

(*b*) Very faint, 8′ or 10′ following small star. This nebula is 20′ ± north preceding a larger nebula, whose approximate place is R.A. $3^h\ 14\frac{1}{2}^m$, Dec. −19° 49′ (1885·0). I have taken this latter nebula to be General Catalogue 689, with an error of one degree in declination, inasmuch as I find no nebula in the place of G. C. 689. If it be that nebula, and I feel certain it is, the place in Gen. Catalogue is too far south by approximately 1°.

(*c*) Faint, close to, and south preceding a small star; the star is partly involved in the nebula.

(*d*) Very faint, close between two bright stars, the *n.p.* of which is about 8 mag., the *s.f.* is about 9 mag. A 10th mag. star is involved in the following edge of the nebula.

The above places are quite close, and have been determined with the 6-inch Cook Equatoreal of this Observatory.

COMET BARNARD II. 1884.—This comet was last observed here with the 5-inch refractor on the nights of November 5, 6, 7, 8, 9, 10, 11, 1884. On the last date it was *extremely faint*, a mere breath of the dimmest haze. It could have been followed, however, longer; but the night of November 12 was cloudy, and that of November 13 bad, and I could not again find it.

Two small Red Stars.—A notice of three small Red Stars appeared in the 'Sidereal Messenger,' No 29. I understand that this was copied in the Jan. number of 'The Observatory.' A typographical error having occurred in the declination of the first star, wherein $+30^\circ\ 39'\cdot1$ was printed for $+20^\circ\ 39'\cdot1$, I desire to make a correction. The closely approximate places are

$$\left.\begin{array}{l}\alpha=5^h\ 38^m\ 43^s\cdot4\\ \delta=+20^\circ\ 39'\cdot1\end{array}\right\}\ 1885\cdot0$$

and

$$\left.\begin{array}{l}\alpha=18^h\ 58^m\ 12^s\cdot2\\ \delta=-5^\circ\ 59'\end{array}\right\}\ 1885\cdot0$$

Mr. Chandler has kindly informed me that these stars are in Birmingham's Red Star Catalogue. E. E. Barnard.

Observatory of Vanderbilt University,
Nashville, Tennessee, 1885, Feb. 25.

On the Defining Powers of Large Telescopes.

In Mr. Denning's letter on this subject, in the March number of the 'Observatory,' he advocates the use of instruments of moderate aperture for the observation of planetary markings, in consequence of the great increase of atmospheric disturbance when a large telescope is employed.

I am very conscious of the value of a restricted aperture, as any excess of light beyond that which is sufficient to exhibit the desired details is injurious: still I greatly prefer to have a large instrument at command. My plan is to have a number of stops, cut in thin sheet-zinc, ready at hand; these rest on a ledge just within the mouth of the 18-inch reflector, and regulate the amount of light admitted to the mirror, according to the state of the air or the magnifying-power employed.

With regard to the reduction of aperture by means of stops, some seem to object to this means of improving definition, saying they would not use a telescope that required to be stopped down; but I cannot conceive any reasonable objection to the process, indeed it seems to be at times absolutely necessary. My mirror, by With, is, I believe, as perfect as a mirror can be, from centre to margin, and the optical arrangements are just as good for the restricted as for the full aperture; but on a bad evening every increase in the volume of disturbed air makes "confusion worse confounded," and the only way of escape is by reducing the quantity of light admitted to the instrument. To give a case in point:—On endeavouring to show Saturn to some friends, with the reflector, the air was in such a disturbed state that no focus could be obtained—half a dozen Saturns were engaged in mazy dance, crossing and recrossing each other in a manner I had never seen before; so the reflector was abandoned in favour of a 5-inch refractor, which, rather to my surprise, gave a fairly steady view with a power of 170. After my friends were gone, I returned to the 18-inch, fearing that something had gone wrong with the setting of the mirror, and putting in the smallest stop, an eccentric of 7 inches, I obtained, much to my delight, a view better than that given by the refractor. Now came the test; while the eye was at the instrument the stop was lifted out, thus giving access to the whole of the mirror, when away went the dance as perplexing as before. Therefore on that night the possessors of small instruments certainly had the best of it.

Now for the advantage of size. It frequently happens on evenings when the air is in a fairly steady state that the light is weak, perhaps a slight mist or fog is forming: these are grand opportunities for steady seeing, if only the light is sufficient to bring out the details; it is here that a small telescope fails, whereas with a large one stop after stop is removed and sufficient light retained to continue the observation or drawing. Many times I have succeeded in completing a series of views that must have come to an end had I worked with an instrument of from 6 to 10 inches, as recommended by Mr. Denning. On those rare occasions when the air is both clear and steady, the big instrument simply walks away from the smaller, by the amount of magnifying-power it will bear.

As to eyepieces, I can heartily second the advice to use single lenses; with the exception of the restricted field, every thing else is in their favour. There is a great advantage in the single lens on occasions when the air is clear but unsteady, for as it passes to the eye a greater amount of light than the usual double-lens eyepiece, a smaller stop can be employed with it, thus reducing the disturbance to a minimum.

While on this subject, I cannot withhold the mention of a means I have employed on occasions when no other eyepiece would work—that is, attaching to the telescope the usual binocular microscope, employing a low-power objective, bringing the enlargement up to 200 or 250 diameters. The effect, at least to my eyes, is a wonderful steadying of the image, enabling the eye to examine minute forms that otherwise would have been invisible or only seen momentarily. The binocular has no advantage on a fine evening; it only works well when light is strong, but the air disturbed. How it is that the steadying effect is produced I know not; perhaps some of our friends who are learned in optics would oblige us either with an explanation or speculation on the matter.

N. E. Green.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Comets of 1618.

GENTLEMEN,—

It is well known that there is some confusion respecting the number of comets seen in the year 1618, and whether one at least of those sometimes reckoned as comets was only a meteor. The first comet of the year passed its perihelion in August, and was observed for about a month, beginning shortly after the perihelion passage. The last comet of the year was discovered at the end of November, was a very splendid object, and was observed until January 1619. These two alone were sufficiently observed to make it possible to determine their orbits. The last passed its perihelion about the 8th of November, soon after which another comet was seen in Persia, the tail only becoming visible in Europe. This was the comet which Kepler surmised might have divided into two, the supposition deriving some plausibility from a similar conjecture having been formed long before with regard to the famous comet of Aristotle. It is, however, doubtful whether this might not have arisen from some confusion with a meteoric observation in November 1618.

But I should like to be allowed to ask a question concerning an observation of an object seen somewhat earlier in that year, respecting which Pingré says, "La prétendue comète qu'Ursin dit avoir été vue le 30 Octobre à Cologne au nord-est et qu'il dit n'avoir pas duré long-temps, étoit aussi sans doute un météore." This statement is evidently taken from Kepler, who writes as follows:—"Quod igitur de 30 Oct. Colonia Agrippina perscriptum commemorat Ursinus, visum esse cometam ad Borrhapelioten tractu caudæ longo, qui non diu duraverit, id ante de hoc meteoro quod ante 10 dies in Wirtembergia visum, accipiendum esse." Now the author to whom he refers is undoubtedly B. Ursinus, who published at Berlin, in 1619, a tract entitled "Ausführliche Bericht von den Cometen . . . . . ." Unfortunately I do not think there is any copy of this treatise in England, although the author's mathematical works are in the library of the British Museum. Nor is it even mentioned in Poggendorff's 'Handwörterbuch,' though it is mentioned in Lalande's 'Bibliographie Astronomique.' Kepler's identification of the supposed comet with a meteor seen by his friend W. Schickard 10 days before is amusing. But whilst he is careful to say that Schickard's observation was made Oct. 10 (O.S.) and 20 (N.S.), it does not seem certain from the quotation whether Oct. 30 in Ursinus is to be taken O.S. or N.S.; if the former it was twenty, if the latter (as is most probable), ten days after Schickard.

Newton, in the third book of the 'Principia,' suggests that the object thus observed at the end of October, which he calls the second comet of 1618, may have been identical with the "fourth or last of the year," *i. e.* the fine comet discovered on November 30, some time after its perihelion passage, before which it may have been seen, he thinks, in the early morning.

It is difficult to form an opinion on this without access to the tract of B. Ursinus to which I have referred; and I write in the hope that some of the scientific foreign readers of the 'Observatory' may be able to consult it and give the quotation.

Yours faithfully,
W. T. LYNN.

Blackheath, 1885, Mar. 7.

Note on a possible case of Proper Motion in a Nebula.

GENTLEMEN,—

The nebula G.C. 2091, if the earlier results can be trusted, seems to afford a marked example of proper motion. It is described by Sir John Herschel as surrounding his double star No. 2529, and was carefully observed by him on two occasions in 1830 and once in 1831. It is entered in his fifth Catalogue as follows:—

A *a* 95°·3 : 1″–1½″ ± : 10½, 12.
A B 8°·6 : 18″ ± : ,, 13½.
A C 230° ± : 6″ ± : ,, 14½.

(I have added the letters A, *a*, B, and C for the sake of convenience, have altered an obvious misprint of 330° for 230° in the angle of A C, and have given the estimate of distance on April 6, 1831, for A B. This is evidently the correct value, and the two estimates given in March 1830 are underrated.) Herschel calls it "A most curious, delicate, and interesting object, the nucleus of a very faint nebula, examined with 320; proves to be distinctly a 1st class double star. . . . A minute and very close double star forms the nucleus of a small round nebula. One or two other small stars in the vicinity seem unconnected with it; but the exactly central position of the double star strongly points to a physical relation between them."

Professor d'Arrest examined this object in 1864 and 1865 with the 11-inch refractor. At the date of his observations B was in the centre of the nebula, A being situated just on its southern edge.

On April 2, 1872, the nebula, as measured at Parsonstown, was 19″·7 from A on an angle of 13°; and again, on March 22, 1876, the nebula is described as a very faint nebulosity around B (at 5°·0 : 21″), elongated suddenly towards the north.

The nebula was found by Mr. Burnham in 1878 and 1882 to be at 18″·96 distance from A, on an angle of 7°·9.

The relative position and distance of A B appear to have been unchanged from 1830 to 1882, while the nebula, which in 1830 and 1831 surrounded the close pair *a*, so that it appeared exactly in the centre, had moved by 1864–5 to the *nf*, so that B was now in the centre and A only on the south margin; while in 1872–1882 it had so far receded that B, instead of being at the centre, was then just at the S. margin, as A had been in 1865, and A itself was 19″ to the south of the nearest portion of the nebula. Irregular but certain motion seems well established. The star C is very probably variable. It is rated 10½ mag. by the Parsonstown observer in 1876, 11 mag. on February 6, and 15 mag. on March 16, 1865, by d'Arrest, while Mr. Burnham found it "excessively faint" in 1878 with the 18½-in. at Chicago.

The place of the nebula for 1885·0, brought up from the G.C., is $\mathrm{x}^{h}\ 17^{m}\ 35^{s}\ +13^{\circ}\ 8'{\cdot}1$. It is No. 705 of the Catalogue of 1833.

H. Sadler.

32 Manor Street, Clapham,
1885, March 19.

Variable Stars.

Gentlemen,—

In a letter to 'The Observatory,' which appeared in the number for August last, I gave the results of a series of observations of the variable star S Cygni extending over a period of twenty years. During the former part of the period the agreement between the observed and calculated times of maximum was close; but the last six years of it showed a change in this respect, the observed being in advance of the predicted times, the difference amounting to 30·3 days at the maximum of May 6, 1881.

It was therefore a point of interest to observe how matters would stand at the maximum, which was due, according to Prof. Schönfield's elements, on the 17th of last December. My observed time of maximum was December 1, sixteen days earlier than the predicted time, and the observed magnitude was 10·1. The maximum was also well observed by Mr. Baxendell, who kindly allows me to quote his result, viz. Max. December 4, mag. 10·1,—three days later than my observed date, but still thirteen days in advance of the ephemeris. This maximum was in magnitude a rather low one.

I have been able to observe partially two minima of S Cancri, namely on February 20 and March 11. As the star on each occasion neared the horizon before it had rounded the curve, I am unable to give the precise times of minimum; but it seems probable that the predictions of your published ephemeris are pretty correct.

Yours faithfully,
George Knott.

Knowles Lodge, Cuckfield,
1885, March 19.

Occultation of Aldebaran.

Gentlemen,—

Heavy rain and clouds all afternoon. At 5.30 G.M.T. these rolled away, and disclosed the Moon in a perfectly clear sky, too late, however, to witness the immersion.

The emersion, however, was instant, sharply defined, and the red colour of star in beautiful contrast to the pale edge of the Moon.

Local S.T.	$3^{h}\ 43^{m}\ 33^{s}$
Add diff. Long. Greenwich	9 31·45
Greenwich Sidereal Time	3 53 04·45

J. L. Stothert.

Audley Park Gardens, Bath.
1885, Feb. 22.

Messier.

GENTLEMEN,—

With reference to your remarks in the "Notes" in the current number of the 'Observatory,' that the Rev. S. H. Saxby wishes attention drawn to the existence of a depression S.E. of Messier B, I may state that, although not in the charts, this formation is not new to me. I have a sketch in my note-book bearing date July 2, 1870 (morning light), with the remark, "A shallow depression on the S.E. of Messier B." Likewise a sketch showing a large shallow circular depression on March 23, 1874. I also saw the same object in the late sunrise on the craters.

May I ask if a little crater on the N.E. of B, discovered by myself Aug. 9, 1865 (in the evening light on the crater), and only imperfectly recovered since on the two dates given above, has been seen by any one else? It has seemed to me sometimes inside and at other times *outside* the crater wall. Yours truly,

Champion Hill, S.E. HERBERT INGALL.
1885, Mar. 2.

NOTES.

DETERMINATIONS OF LONGITUDES IN SOUTH AMERICA.—We learn from 'Science' (vol. v. p. 151) that Lieut.-Commander Davis, U.S.N., has recently completed the telegraphic determination of longitudes on which he has been engaged on the western coast of South America. The great chain of longitude measurements connecting Washington with various points in South America, thence to St. Vincent, St. Vincent to Madeira, Madeira to Lisbon, Lisbon to Greenwich, Greenwich to Washington, has now been completed. This chain, consisting of twenty links, closes with but an insignificant discrepancy: the longitude of the Cordoba Observatory by way of Lisbon, Rio de Janeiro, and Buenos Aires being $4^h\ 16^m\ 48^s{\cdot}06$, and by way of Washington, Panama, and Valparaiso $4^h\ 16^m\ 48^s{\cdot}24$, showing a discrepancy of only $0^s{\cdot}18$. This affords a remarkable proof of the accuracy of the methods and instruments now in use for such operations.

RAMBLES THROUGH STARLAND *.—The difficulty of writing a popular work on a scientific subject has been so generally recognized, that it is almost sufficient commendation of any book which professes to be of that class to say that it even passably fufils its mission; for if written by a purely literary man, it is liable to contain blunders—if by a scientific man to be too technical. With regard to the little book before us, we have no hesitation in saying that it is deserving of high rank among popular works on Astronomy. Consisting as it does of magazine articles which are consequently written in a style likely to be attractive to the ordinary reader, it yet never offends against scientific accuracy, and is obviously the work of a man who "knows his subject." As one might expect from the original form in which it appeared, it is far from being a connected treatise on Astronomy; the "rambles" are taken over very varied ground; the author tells his quite unscientific readers of the difficulties of a scientific life in general, and of an astronomical life in particular; or gives them some information as to how clocks are regulated by the Earth's revolution; or, again, describes an earthquake with many poetic images; or wanders further afield into other scientific "mysteries," such as the wonderful working of electricity; but through it all he takes care to make his statements both correct and interesting; and it is not easy to find terms in which to express more emphatic praise for a work of this kind.

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* Spaziergänge durch das Reich der Sterne. Von M. Wilhelm Meyer. (Vienna: A. Hartleben, 1885.)

APPARENT ENLARGEMENT OF CELESTIAL OBJECTS NEAR THE HORIZON.—M. Paul Stroobant has recently discussed the cause of this well-known, but little understood, phenomenon in a pamphlet entitled, "The apparent Enlargement of the Constellations, the Sun, and Moon, when near the Horizon." He summarily disposes of the theory of "atmospheric refraction;" the theory that the effect is due to comparison with terrestrial objects is disproved by the fact that the phenomenon is observed on the open ocean, and also by experiments with dark glasses which allow only the Sun to be seen. Experimenting with electric sparks placed on the wall and ceiling of a large hall, M. Stroobant found that if the two sparks overhead were 100 mm. apart, those on a level with the eye need only be 81·5 apart in order to appear as widely separated. Experiments with stars in the zenith and near the horizon gave a very similar result, viz. a separation of 100 units between two stars in the zenith appeared no greater than a separation of 79·7 between two on the horizon. The concordance between these independent determinations shows that the supposed apparent "ellipticity of the celestial vault" is not the true cause of the appearance in question. M. Stroobant is disposed to think that the loss of brilliancy suffered by the Sun, and yet more notably by the Moon when low down, has much to do with the phenomenon, but that it is mainly due to some physiological cause, connected with the direction of vision, worthy of further and special study.

THE EMPLOYMENT OF APPROXIMATE FORMULÆ IN THE REDUCTION OF TRANSIT OBSERVATIONS *.—M. Loewy has recently discussed the important question as to what are the limits of magnitude of the instrumental errors of a transit, in order that no appreciable error may affect the results from the use of the ordinary approximate formulæ. Adopting the usual notation the formulæ are:—

$$\tau - m = n \tan\delta + c \sec\delta \quad \ldots\ldots\ldots\ldots \quad (1)$$

$$n = b \sin\phi + a \cos\phi \quad \ldots\ldots\ldots\ldots \quad (2)$$

$$m = b \cos\phi + a \sin\phi \quad \ldots\ldots\ldots\ldots \quad (3)$$

* Comptes Rendus, Tome c. Nos. 3 & 4.

Now if we suppose that the limit of accuracy to which we wish the reductions to be carried to be ϵ for an equatoreal star, we may assume that this quantity will become $\epsilon \sec \delta$ for a star of declination δ. Confining himself, in the first instance, to the quantities n and c (which are practically of the same order of magnitude, so that $n/c = \pm 1$) M. Loewy finds that for stars within 20° of the pole, with which we are most concerned in an enquiry of this nature, formula (1) is sufficiently accurate provided that c or $n \leqq \frac{45^{s}\cdot 6}{\sqrt[3]{\sec^{2}\delta}}$, it being assumed that $\epsilon = 0''\cdot 01$. Applying this to *Polaris* and to λ *Ursæ Minoris*, it appears that the superior limits of the instrumental errors in question are $3^{s}\cdot 6$ and $3^{s}\cdot 2$ in each case respectively. If this condition be fulfilled the error of reduction will not exceed $0^{s}\cdot 03$ for *Polaris*, and $0^{s}\cdot 04$ for λ *Ursæ Minoris*.

Proceeding to the discussion of formulæ (2) and (3), and assuming that a and b are quantities of the same order of magnitude, so that we may put $a/b = \pm 1$, the necessary condition in order that these formulæ may be sufficiently accurate for all practical purposes is a or $b \leqq \frac{58^{s}}{\sqrt[3]{\cos \phi}}$. In this expression the same assumption is made as before with regard to the magnitude of ϵ. The two very convenient expressions for the limiting values of c or n and of a or b which M. Loewy has deduced will doubtless be of great service to practical astronomers, especially to those who are engaged in the determination of the positions of close polar stars.

TOTAL LUNAR ECLIPSE OF OCT. 4, 1884.—We gather, from a letter in the 'Astronomische Nachrichten,' that Herr Struve has received the results of observations during the total eclipse of the Moon on Oct. 4, as obtained at 36 stations. In all, he has at his command the times of 512 phenomena. As might be expected, the observed re-appearances are less in number than the disappearances. Nevertheless he has sufficient data to give the mean diameter of the Moon at that epoch with great accuracy. No. 85 in Dr. Döllen's list gives an especially rich series, owing probably to the circumstance that the phenomena occurred about the middle of the eclipse.

A strict determination of the diameter and figure of the Moon cannot be made until the places of the occulted stars are accurately known. If this work is undertaken by Pulkowa alone, it will occupy at least 2 years. Co-operation is therefore necessary. With the hope that astronomers will assist in this object, he publishes a list of 45 stars with their magnitudes and catalogue places. As most of the stars are very faint, comparisons with brighter stars by means of an equatoreal and micrometer will be requisite.

Orbits of Meteors.—The following Meteor Orbits[*] have been computed by Mr. O. C. Wendell, of Harvard College Observatory, from radiants deduced by Messrs. Denning and Sawyer. For observations see 'Monthly Notices,' Vol. xlii. No. 2, p. 85, and 'American Journal of Science,' Vol. xvii. June.

No.	Catalogue and No.	Long. of Perihel.	Long. of Node.	Inclin.	Perihel. Distance.	Motion.
1.	Denning, No. 1...	265°·9	129°·0	77°·4	0·865	Direct.
2.	,, ,, 4...	291 ·0	129 ·0	85 ·7	0·976	,,
3.	,, ,, 9...	299 ·2	123 ·3	64 ·2	0·999	Retrograde.
4.	,, ,, 11...	300	126 ·0	34	0·997	,,
5.	,, ,, 14...	215 ·5	128 ·5	17 ·5	0·474	,,
6.	,, ,, 15...	308 ·5	127 ·0	49 ·0	1·000	,,
7.	,, ,, 16...	326 ·0	121 ·4	79 ·3	0·955	,,
8.	,, ,, 17...	305 ·9	126 ·2	26 ·4	1·000	,,
9.	,, ,, 18...	22 ·0	128 ·5	81 ·7	0·642	,,
10.	,, ,, 19...	46 ·0	126 ·2	80 ·0	0·415	Direct.
11.	Sawyer, ,, 16...	140 ·5	188 ·2	16 ·6	0·163	,,
12.	,, ,, 17...	100 ·3	185 ·7	20 ·6	0·460	,,
13.	,, ,, 20...	129 ·8	206 ·5	28 ·1	0·385	,,
14.	,, ,, 22...	115 ·9	211 ·5	7 ·7	0·549	,,
15.	,, ,, 27...	170 ·7	243 ·1	37 ·2	0·349	,,

The orbit computed from Sawyer radiant No. 22 bears a close resemblance to that of the comet of 1757, and indicates their probable identity.

The Influence of Temperature on the Character of the Lines of the Spectrum.—M. Ch. Fievez, Astronomer in charge of the Physical Department at the Royal Observatory, Brussels, has forwarded us an interesting and important pamphlet, containing two papers on the above subject which he has communicated to the Royal Academy of Belgium, together with the report of M. Stas upon them. It is M. Fievez's conclusion from a series of experiments upon hydrogen gas that the widening of the lines of this gas is "*independent of the pressure and correlative to a rise* of temperature." For, as he goes on to say, "When the electric spark is employed to carry a chemical element to the state of vapour or of incandescent gas, every modification in the resistance of the gaseous medium effects a modification of the temperature of this medium if the other conditions do not change. This temperature can be raised by increasing the pressure, by introducing a condenser into the circuit, or by increasing the surface of the condenser, and it can be lowered by the reverse means." M. Fievez therefore does not mean to allege that the lines of hydrogen would not be widened by increasing the pressure, but that the effect is an *indirect* one, and is due to the greater tension of the spark and the consequent rise of temperature. This conclusion is borne out by a number of experiments, the most interesting of which illustrate in a remarkable manner the curious phenomenon of *double reversal* which has been observed on several occasions in the *b* and D lines at the base of a prominence, and especially by Prof. Young, who has described the appearance very clearly. The dark lines of sodium or magnesium on such occasions are seen to be greatly broadened, then a bright line appears in the middle of each dark line, and sometimes a dark line again appears in the centre of this bright line. One of these experiments is described as follows :—"Some fragments of sodium were placed in a platinum tube of 50 mm. in length and 10 mm. in diameter, arranged in line with the optical axis of the collimator of the spectroscope. The flame of an oxyhydrogen blowpipe being introduced into this tube the sodium lines D_1 and D_2 shone out immediately with a vivid glow, widened out until they united, a black line appeared in the centre of each of these broadened lines, these black lines widened out until they united, then a bright line appeared in the middle of each of the widened lines, and, lastly, a new dark line showed itself in the middle of these bright lines." Somewhat similar results were obtained with lithium, potassium, thallium, &c. Less striking, but not less important, results were obtained for the C and F lines of hydrogen. The C and F lines obtained from a Geissler tube filled at 1500 mm. pressure were both widened simply by moving the electrodes further apart. A condenser introduced into the circuit produced, not merely the broadening of the F line, but its reversal; with a condenser of greater surface, C also was broadened and a narrow black line appeared in its centre. From these and many similar experiments, M. Fievez concludes that "an increase of complexity in the constitution of a spectral line is a certain index of an increase of the temperature of the emissive vapour." From whence it follows that "the temperature of sun-spots is higher than that of the limb, because the spectral lines of spots have a greater breadth than those of the limb."

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[*] Astron. Nach. No. 2652.

The second paper of M. Fievez is a very brief one, and merely asserts that he has been able to reproduce in the laboratory changes of refrangibility in the lines of hydrogen and magnesium similar to those often witnessed during solar storms. A fuller description of these most important experiments would be very welcome.

AMERICAN ECLIPSE EXPEDITION TO CAROLINE ISLAND, MAY 1883.—The complete report of this expedition to observe the total solar eclipse of May 6, 1883, has recently been published as a memoir by the National Academy of Sciences. The principal astronomical features of the report—viz. Prof. Holden's inability to find any object that could possibly be an intra-Mercurial planet, and Prof. Hastings's remarkable spectroscopic observations, or rather the observations which he cites in support of his remarkable theory as to the non-solar character of the corona—have been already commented on in the pages of the 'Observatory' (Vol. vi. p. 251). But in addition to these the memoir contains an interesting report on the meteorology of the island by Mr. Winslow Upton, reports on the *flora* and *fauna* by other members of the party, and determinations of the geographical position of the observing station. The results of the latter are, for longitude, by transport of chronometers combined with two Moon culminations, $10^{h}\ 0^{m}\ 57^{s}\cdot6$ west of Greenwich; and for latitude, by the method of equal zenith distances, $10^{\circ}\ 0'\ 0''\cdot59$ south. Altogether, apart from its strictly astronomical value, the report contains most interesting information about this almost unknown island, or rather group of islets, and reflects the greatest credit on Prof. Holden as chief of the expedition, and on the other members of the party who worked under him.

STELLAR DISTRIBUTION *.—In commenting on Littrow's discussion of the distribution of the stars of the 'Bonner Durchmusterung' (Astr. Nach. No. 1741), M. Radau gives a very convenient method for formulating the results of investigations of this nature. If we assume that the distances of the stars and their magnitudes form two geometrical progressions of which the ratios are respectively x and $\frac{1}{x^2}$, and that the sphere of radius r contains a number of stars proportional to r^3, the successive sums will give a third progression of which the ratio will be x^3. Not taking into account stars fainter than the 9th mag., we have, from Littrow's enumeration, in the mean $x=1\cdot53$, $x^2=2\cdot35$, $x^3=3\cdot6$. According to Pogson's investigations, $x^2=2\cdot5$, $x^3=4$.

SOLAR PARALLAX FROM PHOTOGRAPHS OF THE TRANSIT OF VENUS 1874 †.—M. Obrecht has published the results of his discussion of the measures made by MM. Baille and Gariel of the daguerrotypes of this Transit of Venus obtained by the French astronomers. He finds for the solar parallax

$$8''\cdot80+0''\cdot004\delta L+0''\cdot004\delta Y,$$

where δL is the error of the assumed longitude of Pekin, δY an error of the tables. By varying δL between $+5^{s}$ and -5^{s}, and δY between $+2''$ and $-2''$, the resulting parallax varies between $8''\cdot77$ and $8''\cdot83$.

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* Bulletin Astronomique, Tome ii. p. 88

† Comptes Rendus, Tome c. No. 4.

THE DIAMETER OF MARS*.—At the opposition of 1881–82 Prof. C. W. Pritchett obtained an extensive series of measures of the diameter of Mars, with the filar-micrometer of the refractor of the Morrison Observatory. Comparing these with a corresponding series made by him near the time of opposition in 1879–80, Prof. Pritchett finds :—

Directions.	Equator.	Eq.+45°.	Poles.	Poles+45°.
Results for 1879–80...	9″·638	9″·517	9″·422	9″·489 (at unit of distance)
Results for 1881–82...	9 ·635	9 ·414	9 ·394	9 ·449 (at unit of distance)

Neglecting the ellipticity of the disk (of which both series give decided indications), there results for the general mean for 1879–80 9″·486±0″·033, and for 1881–82, 9″·484±0″·036. Prof. Pritchett points out that his mean result does not differ much from the arithmetic mean between Herr Hartwig's value from heliometer measures, 9″·352 (Publ. Ast. Ges. xv.), and that deduced by Mr. Downing from 537 measures of the vertical diameter made with the Greenwich Transit-circle between the years 1851–1880, viz. 9″·697 (Monthly Notices, vol. xli. p. 42). With regard to the indications of ellipticity which appear in these measures, it may be pointed out that it is very desirable that astronomers, when making observations of this kind, should employ a reversing prism, as has recently been done by Prof. Seeliger ('Observatory,' Vol. vii. p. 302), in order to eliminate systematic errors depending on the direction of measurement.

THE DEARBORN OBSERVATORY.—The Report of the Director of the Dearborn Observatory for the year 1884 has been issued. From it we learn that the principal instruments, viz. the great equatoreal and the Repsold Meridian Circle, have been kept in constant use during the year. With regard to the former, Prof. Hough states that a gas-engine has been placed in the tower for turning the dome. It has a working-power of 8000 foot-pounds, or about one-fourth horse-power, and appears to act very satisfactorily. The observations made with the great equatoreal have been mainly of (1) Pons-Brook Comet; (2) Difficult double stars; (3) the planet Jupiter; (4) the Satellites of Uranus; (5) Miscellaneous Observations. These observations have been made, as heretofore, by Prof. Hough in conjunction with Mr. S. W. Burnham. The companion of Sirius was measured on eleven nights by Prof. Hough, and on ten nights by Mr. Burnham; the mean results of the two sets are respectively :—

Date.	Position-angle.	Distance.
1884·179........	36°·7	8″·51
1884·191........	36 ·4	8 ·39

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* Astronomische Nachrichten, No. 2652.

The observations of Jupiter given in the Report have been ably discussed by Mr. Denning ('Observatory,' Vol. viii. p. 76), and need not be further alluded to here. But few measures of the satellites of Uranus have been obtained, the weather during the spring having been generally unfavourable for such difficult observations. On nearly every clear day or night, meridian observations for time have been made with the Repsold Meridian Circle, and standard time furnished to the city of Chicago daily.

The Observatory under Prof. Hough's direction appears to be doing good work, which is doubtless considerably curtailed in amount by the want of a sufficient staff of observers to carry out a more extensive programme.

THE WASHINGTON OBSERVATORY.—In accordance with the intention notified in the last Annual Report of the Naval Observatory ('Observatory,' Vol. viii. p. 56), the Superintendent, Commodore Franklin, has issued a programme of the work to be carried out during the current year. With the great equatoreal measures of a selected list of double stars, showing rapid motion or other peculiarity, are to be continued; the conjunctions of the inner satellites of Saturn will be observed, and a complete micrometrical measurement of the rings executed. Observations which have been commenced for stellar parallax will be finished. The Transit-Circle is to be employed on observations of the Sun, Moon, and larger planets, the latter being observed from fifteen to twenty times near opposition, and, in addition, each minor planet will be observed at least five times, if practicable, near opposition. The 9·6-inch equatoreal will be utilized for observations of all the minor planets whose brightness at opposition is greater than their mean brightness, for positions of comets, and for occultations. The prime-vertical transit instrument is to be used in observing a selected list of stars in conjunction with the Royal Observatory at Lisbon, in pursuance of a plan recommended by the International Geodetic Association, for the determination of variability of latitude. With the mural circle observations will be made of stars down to 7th magnitude, south of 10° north declination, the positions of which have not been recently determined at any northern observatory; the Observatory list including stars in Gould's 'Uranometria Argentina' not found in Yarnall's Catalogue, the transit circle list of B.A.C. stars, or the recent catalogue of the Glasgow Observatory.

THE MELBOURNE OBSERVATORY.—The nineteenth Annual Report of the Government Astronomer has been published. Mr. Ellery announces that the new transit circle of 8 inches aperture, constructed for the Observatory by Mr. Simms, was received in May last, and the mounting completed early in July. It is proposed to send the two specula of the great reflector, one after the other, to England, to be repolished. Observations and drawings of Herschel's nebulæ, among them the nebula of η Argûs, 30 Doradus, and the "Horseshoe" nebula, had been made during the year with this instrument. A number of stars selected by Prof. Auwers had been observed with the old transit-circle, to assist in the formation of a fundamental catalogue of southern stars. The completion of the telegraphic determination of Australian longitudes, it is reported, was only waiting a new series of exchanges between Sydney, Adelaide, and Melbourne. New Zealand had been connected with Sydney by a most successful set of time-exchanges through the cable. The connection of Brisbane and Sydney was in progress, and, on this being completed, there would only remain to connect Western Australia, to have the longitudes of all the chief Australian and New Zealand cities and ports determined upon the same system.

Mr. Ellery recommends that a small expedition should be despatched from Melbourne to New Zealand for the observation of the total eclipse of the Sun on Sept. 9 in the present year, when the central line passes through Cook's Straits. Sir W. Jervois, Governor of New Zealand, had promised all the aid he could render in the matter. The Board of Visitors supported an application to the Government of Victoria for the necessary funds.

THE HONGKONG OBSERVATORY.—We have received Dr. Doberck's Annual Report as Government Astronomer for Hongkong for the year 1884. From it we learn that this Observatory, though still in its infancy, is doing work, chiefly in the department of Meteorology, that from its quality and quantity would be highly creditable even to an old-established institution. We understand that Dr. Doberck has secured an equatoreal telescope for his Observatory, and we hope before long to find that astronomical observations form not the least important part of his multifarious duties.

THE HARVARD COLLEGE CATALOGUE OF 1213 STARS FOR 1875.—This catalogue contains the results of observations made during the years 1870 to 1879 with the meridian circle of the Harvard College Observatory, the stars observed being chiefly those required for the revision of the 'Durchmusterung' for the zone between the parallels of declination +50° and +55°. The star places employed as points of reference were taken from the list given in Publication XIV. of the 'Astron. Gesellschaft.' The right ascensions and declinations published in the catalogue are derived from a discussion of the results obtained during the whole period covered by the observations. They are given in their present form for the purpose of making them immediately available in advance of the publication of the volume which is to furnish the details of the investigation, viz. Volume XV. of the 'Annals.' The observations were nearly all made by Prof. Rogers, who has also had charge of their reduction and publication—a statement which is a sufficient guarantee for the accuracy of the observations and for the thoroughness of the reductions (*cf.* 'Observatory,' Vol. v. p. 377). Prof. Rogers has communicated to us the following list of errata (which will be corrected in the final volume), and will be glad to receive notification of any others which may be detected:—

No. 129 for Dec. $+19^\circ\ 15'\ 17''{\cdot}60$ read $+19^\circ\ 15'\ 7''{\cdot}60$.
No. 144 for R.A. $3^{h}\ 23^{m}\ 45^{s}{\cdot}420$ read $3^{h}\ 23^{m}\ 58^{s}{\cdot}420$.
No. 395 for R.A. $8^{h}\ 49^{m}\ 54^{s}{\cdot}968$ read $8^{h}\ 48^{m}\ 54^{s}{\cdot}968$.
No. 413 for Mean Epoch of Obs. 1872·1 read 1874·1.
No. 531 for Dec. $+88^\circ\ 24'\ 33''{\cdot}23$ read $+88^\circ\ 23'\ 33''{\cdot}23$.

The entire catalogue contains 1213 stars, and is preceded by the annual results for the fundamental stars.

ROTATION OF DOMES.—Mr. D. P. Todd writes to 'Science' on this subject, to the following effect:—"The chief objection urged against the mounting of rotatory domes on cannon-balls is the difficulty experienced in keeping the balls at equal distances apart. If the dome is much used, this objection becomes a serious one; and no dome so large that it would require more than four balls should be mounted in this manner. If the sill and the bed-plate of the dome are so well built that they retain their figure sensibly perfectly, and the track is kept thoroughly clean, the balls will ordinarily not be found to change their relative position very much, except during the winter season. At this time of year, and under favourable conditions of temperature, the fine snow which is often driven into the observatory, underneath the dome, will, if allowed to remain in the track, form an icy coating over the balls as they pass through it, no matter what the weight of the dome may be. Under such conditions, if the dome is forcibly moved, the incrusted ball will often change its relative position several feet, thereby perhaps imperilling the safety of the dome." Our experience at Greenwich fully confirms Mr. Todd's objections to this mode of mounting domes.

CHANGE IN THE ASTRONOMICAL DATE.—Commodore S. R.

Franklin, Superintendent of the U.S. Naval Observatory, Washington, writes to the 'Sidereal Messenger' to the following effect:—"After consultation with the principal astronomers throughout the country, it has been decided that this Observatory will make no change in the beginning of the astronomical day, the weight of opinion being that it will be better to defer such a change until the ephemerides are constructed in accordance with the recommendation of the recent International Meridian Conference."

VARIATIONS IN THE SPECTRUM OF β LYRÆ[*].—In the summer and autumn of 1883, Dr. N. de Konkoly and Herr E. von Gothard detected a certain amount of variability in the C, D_3, and F lines in the spectra of γ Cassiopeiæ and β Lyræ, in which they are frequently seen to shine as bright lines. During 1884, Herr von Gothard has followed up his investigations, and has observed the spectrum of β Lyræ on thirty nights. On nine of these occasions the D_3 line was seen to be more or less bright; on July 13 it is even described as "almost dazzling"; on several other nights its presence was only suspected; on ten nights no trace of it was seen,—so that it presented on different occasions every possible degree of change of brightness from great brilliancy down to total disappearance. The C line was also seen to be reversed on many occasions, and it was frequently very brilliant; but on others it was only faint, was difficult to see, or was evidently a dark line. The F line was not so often noticed to appear bright; its reversal would naturally be less easily observed than that of C or D_3. Herr von Gothard was not able to satisfactorily determine the length of the period of variability; it was evidently very short, and a period of about seven days seemed to suit the observations fairly well. It should be added that the three lines appeared to vary in perfect accord with each other. It is to be hoped that many observers will be led by Herr von Gothard's remarkable observations to devote attention to the spectrum of this star, in order that the exact character and period of these variations may be determined.

MINOR PLANETS.—A new Minor Planet, No. 245, of 12–13 magnitude, was discovered by Mr. Pogson at Madras while searching for Isis on Feb. 6; it was observed on succeeding evenings by Mr. Pogson and at Vienna on March 9. Another, No. 246, was discovered by M. Borrelly at Marseilles on March 6, magnitude 11. A third, No. 247, was discovered by Dr. R. Luther at Düsseldorf, on March 14, magnitude 11. No. 244 has been named Sita.

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[*] Astron. Nachr. Nos. 2651–2

Liverpool Astronomical Society.—We regret that the report of the last Meeting arrived too late for insertion *in extenso*. A paper by Dr. Klein was read, on the dark spots between Copernicus and Gambart. The President presented a list of 32 red stars not found in any red-star catalogue, several being discovered by photography. Prof. Piazzi Smyth described the phenomena observed by him at the last occultation of Aldebaran: the star suddenly disappeared, but almost immediately reappeared for 2·6 seconds. In submitting the report of the Planetary Section, Mr. Franks mentioned that it contained 38 sketches of Saturn. Messrs. Leigh and R. C. Johnson described the transit of Jupiter IV. on February 27; it appeared of a dark brown colour, and could be distinguished from the shadow only by its size.

Prof. R. Wolf has been elected a Corresponding Member for the section of Astronomy of l'Académie des Sciences.

We learn from the 'Athenæum' that Dr. Samuel Rowbotham, better known as "Parallax," is dead.

Elements and Ephemeris of the periodic Comet Tempel II. 1867 *for its return in* 1885. By Raoul Gautier.

The calculation of the perturbing action of Jupiter upon the comet during the period 1879 to 1885 has given for the principal results a retardation of 148 days in the epoch of perihelion passage, a sensible augmentation of the semi-major axis, and a diminution of the eccentricity. The following are the elements for the apparition of 1885.

Elements for 1885, osculating Sept. 19, 1885.

Sept. 19·0	−1° 1′ 28″·94.	
T	Sept. 25·7649, Berlin M.T.	
μ	545″·3073	
ϕ	23° 53′ 57″·0	
i	10 50 27 ·2	Mean Eq. 1885·0
☊	72 28 7 ·7	
ω	168 57 53 ·3	
log a	0·542244	

The following ephemeris will enable the comet to be sought for in that part of the month of April when moonlight will not interfere with observations. The theoretical brightness of the comet T has been calculated by taking as unity the brightness it had at the time of the last observation made by M. Tempel at Florence, July 8·5, 1879, for which date $\log \Delta = 9·9713$ and $\log r = 0·2677$.

Ephemeris for Berlin Midnight.

	R.A. 1885·0.	Diff.	δ 1885·0.	Diff.	log Δ.	log *r*.	T.
	h m s	m s	° ′	′			
April 2·5	11 55 17		+18 56·3		0·1792	0·3877	0·221
		−1 33		+ 1·9			
4·5	53 44		58·2				
		1 30		0·9			
6·5	52 14		59·1		0·1803	0·3851	0·222
		1 27		− 0·2			
8·5	50 47		58·9				
		1 23		1·2			
10·5	49 24		57·7		0·1824	0·3826	0·223
		1 19		2·4			
12·5	48 5		55·3				
		1 13		3·5			
14·5	46 52		51·8		0·1855	0·3801	0·222
		1 9		4·6			
16·5	45 43		47·2				
		1 3		5·6			
18·5	44 40		41·6		0·1894	0·3776	0·221
		0 57		6·7			
20·5	43 43		34·9				
		0 51		7·8			
22·5	42 52		27·1		0·1940	0·3751	0·219

Approximate Times of Transit of the great Red Spot across the Central Meridian of Jupiter in April 1885.

	h		h		h
Apr. 1....	6·7	Apr. 12....	10·8	Apr. 23....	14·8
2....	12·5	13....	6·6	24....	10·7
3....	8·4	14....	12·4	25....	6·5
4....	14·1	15....	8·3	26....	12·3
5....	10·0	16....	14·1	27....	8·2
6....	15·8	17....	9·9	28....	14·0
7....	11·6	18....	15·7	29....	9·8
8....	7 5	19....	11·6	30....	15·6
9....	13·3	20....	7·4	May 1....	11·5
10....	9·1	21....	13·2	2....	7·3
11....	14·9	22....	9·1	3....	13·1

The red spot was observed here as it passed the central meridian on Feb. 25 at 12^{h} 50^{m} (Long. 317°·5), on Mar. 7 at 11^{h} 3^{m} (Long. 36°·7), and on March 15 at 7^{h} 38^{m} (Long. 98°·9).

The *f* extremity of the spot is very distinct, so also is the *f* side of the depression in the dark belt; but the opposite side continues to fade away. The rotation period of the spot, so far as the observations have progressed during the present opposition, is nearly identical with that during the two preceding years. In 1882–3 and 1883–4 it was 9^{h} 55^{m} 39^{s}·1, whereas from Sept. 21, 1884, to March 15, 1885 (422 rotations), it was 9^{h} 55^{m} 38^{s}·6.

There are two equatoreal white spots preceding Mr. Marth's 1st meridian 25 min. (Long. 345°) and 46 min. (Long. 332°) respectively. They are both rather bright. The immediate region where the old white spot should recently have been visible has been frequently observed here with negative results. A number of similar spots near the equator are being watched with a view to determine their periods, and possibly to remove the doubts as to the continued existence of the object so conspicuously visible throughout the past few years.

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rs in April 1885.

minimum.

5 S Herculis. M.
8 R Boötis. M.
8·5 T Monocerotis, *m.*
8·5 ζ Geminorum, *m.*
20 S Hydræ. M.
22 T Capricorni. M.
22 S Ursæ Majoris. M.
23 R Draconis, M.
23·9 ζ Geminorum, M.
2 R Persei, M.
20 η Geminorum. M.
20 S Scorpii, M.
20·4 T Monocerotis. M.
27 R Coronæ, *m.*
28 T Herculis. M.
28·7 ζ Geminorum. *m.*

Period.

Cephei, *m.*

		h	m
	3	7	54
	8	7	33
	13	7	12

Algol, *m.*

		h	m
	4	17	42
		14	30
	10	11	19
	13	8	8
	27	16	13

S Cancri, *m.*

		h	m
	18	14	8

δ Libræ, *m.*

		h	m
Apr.	3	7	24
	5	15	16
	12	14	50
	19	14	24
	26	13	58

U Coronæ, *m.*

		h	m
Apr.	4	11	22
	11	9	5
	21	17	38
	28	15	21

Astronomical Memoranda, April 1885.

Equation of Time:—Sun *after* Clock, Apr. 1, 3^{m} 50^{s}; Apr. 11, 0^{m} 57^{s}; Apr. 21 (*before*), 1^{m} 25^{s}; Apr. 30, 2^{m} 57^{s}.

Sidereal Time at Mean Noon:—Apr. 1, 0^{h} 40^{m} 2^{s}; Apr. 11, 1^{h} 19^{m} 28^{s}; Apr. 21, 1^{h} 58^{m} 53^{s}; Apr. 30, 2^{h} 34^{m} 22^{s}.

Sun.	Rises.		Sets.		Position-angle of axis.		Heliogr. co-ordinates of centre of disk. Lat.		Long.	
	h	m	h	m	°		°	′	°	′
Apr. 1	17	34	6	31	333	35	−6	24	231	35
6	17	23	6	39	333	30	6	5	165	37
11	17	12	6	48	333	36	5	44	99	36
16	17	2	6	56	333	53	5	20	33	36
21	16	51	7	4	334	20	4	53	327	33
26	16	41	7	12	334	58	4	26	261	28
31	16	32	7	20	335	49	−3	55	195	25

The Moon.

	rises.			*rises.*			*sets.*			*sets.*	
	h	m		h	m		h	m		h	m
Apr. 1	8	48	Apr. 9	15	7	Apr. 17	9	51	Apr. 25	15	36
2	9	53	10	15	35	18	10	58	26	16	2
3	10	53	11	16	2	19	11	58	27	16	28
4	11	48	12	16	27	20	12	50	28	*rises*	
5	12	38	13	16	54	21	13	34	29	7	38
6	13	23	14	*sets*		22	14	10	30	8	41
7	14	2	15	7	23	23	14	41			
8	14	37	16	8	38	24	15	10			

Apr. 7^{d} 2^{h} 42^{m} Last Quarter.
21 11 20 First Quarter.
Apr. 14^{d} 17^{h} 51^{m} New Moon.
28 18 14 Full Moon.

Occultations, April 1885.

Day.	Star.	Mag.	Disap.		Angle.	Reap.		Angle.
			h	m	°	h	m	°
20	λ Geminorum ..	3½	10	59	147	11	20	192
22	B.A.C. 3122	6½	9	4	100	10	4	221
23	π Leonis	5	6	46	28	7	42	291
24	*d* Leonis	5	11	7	109	11	58	206
26	B.A.C. 4255	6½	8	28	85	9	39	225
29	*o*′ Libræ	6	15	46	65	16	55	276

Planets for April 1885.

Day.	Planet.	R.A.	Dec.	Diam.	Rises.	Tr.	*Sets*
		h m s	° ′	″	h m	h m	h *m*
1	Mercury . .	1 45 58	12 46 N.	6·4	17 56	1 6	8 16
11		2 28 3	17 46 N.	8·4	17 30	1 8	8 46
21		2 33 29	17 36 N.	10·8	16 56	0 34	8 12
31		2 14 13	13 23 N.	12·0	16 17	23 30	6 43
1	Venus . .	0 14 23	0 1 S.	9·8	17 31	23 35	5 39
11		0 59 49	4 57 N.	9·7	17 12	23 41	6 10
21		1 45 49	9 44 N.	9·6	16 53	23 47	6 41
31		2 32 58	14 9 N.	9·6	16 37	23 55	7 13
1	Jupiter . .	9 56 35	13 52 N.	39·7	1 59	9 15	16 31
11		9 54 43	14 0 N.	38·7	1 17	8 34	15 51
21		9 54 2	14 3 N.	37·7	0 37	7 54	15 11
31		9 54 33	13 58 N.	36·5	23 54	7 15	14 32
1	Saturn . .	5 12 33	21 53 N.	16·0	20 24	4 32	12 37
30		5 24 9	22 9 N.	15·8	18 39	2 49	10 56
1	Uranus . .	12 2 47	0 33 N.	4·0	5 14	11 21	17 2
30		11 58 50	0 57 N.		3 14	9 23	15 3
1	Neptune .	3 18 18	16 31 N.		19 3	2 38	10
30		3 22 17	16 47 N.		17 11	0 48	8 2

Apr. 7ᵈ 14ʰ. Mercury at greatest elongation (19° 15′ E.).
17 8 Mercury Stationary.
21 15 Jupiter Stationary.
27 15 Mercury in inferior conjunction with the Sun.

Phenomena of Jupiter's Satellites, April 1885.

d	h	m		d	h	m		d	h	m	
1	11	53	ii. Ec. R.	10	10	13	iv. Ec. D.	19	13	12	i. Tr. I.
	14	59	iv. Tr. I.		14	37	iv. Ec. R.	20	10	19	i. Oc. D.
3	15	5	i. Tr. I.	11	14	1	i. Oc. D.		13	45	i. Ec. R.
4	12	13	i. Oc. D.	12	11	22	i. Tr. I.	21	9	59	i. Tr. E.
5	9	32	i. Tr. I.		13	41	i. Tr. E.	22	8	14	i. Ec. R.
	11	52	i. Tr. E.	13	8	29	i. Oc. D.	24	8	34	ii. Tr. I.
6	9	55	i. Ec. R.		11	50	i. Ec. R.		11	30	ii. Tr. E.
	14	28	ii. Tr. I.	14	8	9	i. Tr. E.	26	8	59	ii. Ec. R.
7	10	45	iii. Oc. R.		10	43	iii. Oc. D.	27	8	35	iv. Ec. R.
	11	13	iii. Ec. D.		14	22	iii. Oc. R.		12	11	i. Oc. D.
	14	40	iii. Ec. R.	15	12	0	ii. Oc. D.	28	9	31	i. Tr. I.
8	9	34	ii. Oc. D.	17	9	2	ii. Tr. E.		11	51	i. Tr. E.
	14	29	ii. Ec. R.	18	11	36	iv. Tr. E.	29	10	8	i. Ec. R.

The R.A. and N.P.D. of Planets and the Solar Co-ordinates are given for Greenwich Mean Noon; in all cases Greenwich Mean Time is used, reckoning from noon to noon, as in previous years; the angles for Occultations are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 97. MAY. 1885.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, April 10, 1885.

E. Dunkin, F.R.S., *President*, in the Chair.

Secretaries: E. B. Knobel and
Lieut.-Col. G. L. Tupman.

The Minutes of the last Meeting were read and confirmed.

Captain Noble complained that the 'Monthly Notices' for March had not yet reached him.

Mr. Knobel, after stating that the 'Notices' had been posted the previous evening, said he was glad the subject had been brought before the Fellows, because it was one about which there ought to be a clear understanding. In the preparation of the 'Monthly Notices' there was a good deal of work for which the editors were directly responsible. There was a long diagram in the 'Monthly Notices' just published, which had taken some time to correct and prepare. There were also long tables which had likewise taken time to correct in passing through the press. It was a question entirely for the Fellows to decide whether the 'Monthly Notices' should be hurried through the press so that it might be without fail in the hands of Fellows the day before the ordinary meeting, or rather whether such care should be exercised in passing the 'Monthly Notices' through the press as to ensure the accuracy of their contents. The proofs had not only to be read through, but when they are completed and revised, and passed, and the 'Monthly Notices' had been passed into page, they had to be read through by the Secretaries.

Mr. Chambers said that the explanation which Mr. Knobel had so clearly given had been the periodical explanation during the last twenty years at tolerably regular intervals, and he could not conceive that it would be at all satisfactory to the great bulk of Fellows. Great care should be taken, and he thought great care was taken, in the preparation of the 'Monthly Notices,' but he believed it would be a lesser evil to leave out some of the longer and least important papers and get the 'Notices' out earlier than to delay their publication.

Some further discussion disclosed a general feeling as to the desirableness of earlier publication being secured by holding over the longer and less urgent papers till the next issue.

Col. Tupman announced that 63 presents had been received since the last meeting. Amongst these was a beautiful series of photographs by Mr. Common, who would explain them himself.

The thanks of the Society were voted to the respective donors.

Mr. Green exhibited a drawing of Saturn, which he thought would be interesting in consequence of reported changes on the planet's surface. Accounts had been received from America and France, and from observers at home, that new belts had made their appearance, and that markings previously known had disappeared.

The period of opposition having passed, and there being no equally favourable opportunity for several years, he thought that a careful drawing would be acceptable to the Society, and would serve for reference in the future.

The drawing was made from several studies of Saturn, as seen in an 18-inch reflector, with powers between 250 and 400. He had taken advantage of all the evenings that were favourable, but could not say that good drawings had been the result. In past years he had considered that Saturn would bear a higher power than Jupiter, in consequence of the sharpness of the lines; but during the last opposition he could not get good definition of the rings, and suspected his own eyesight, till turning the instrument on Jupiter, whose altitude at the time was by no means so great, he was able to see the smallest forms with great clearness, and this with the same eyepiece that had failed to give any definite markings on the outer ring, proving that neither telescope nor eyesight were at fault.

The night of the opposition of Saturn in 1883 was more favourable for sharp definition than any he had experienced during the past year. The planet then bore a power of 400 steadily, its details being seen with charming distinctness, and throughout that opposition the markings of the planet generally were more readily observed than in the one that is now past.

Mr. Green remarked that his drawings had the advantage of being carefully compared with the planet by Mr. Edward Nelson, whose eyesight is exceptionally good. He also read a letter from Mr. Arthur Cottam, F.R.A.S., confirming the details of the outer ring.

By the aid of an enlarged drawing on the blackboard Mr. Green then described the appearances on Saturn and the changes as compared with former observations.

Mr. Ranyard said he had observed many of the markings described by Mr. Green, but some of the others he could not corroborate.

Mr. Knobel said there had evidently been a great change in some of the appearances since last February twelve months.

Capt. Noble said it might be interesting to possessors of moderate optical means to know that he had seen the northern belt, as it appeared in the drawing, quite distinctly with a $4\frac{2}{10}$-inch aperture as against Mr. Green's 18 inches. Many other things which Mr. Green saw he could also see with his smaller telescope.

The President. I am sure you will all return thanks to Mr. Green for the interesting manner in which he has gone into his subject. I have myself received a great deal of information concerning the present appearance of the planet.

The President. We may congratulate ourselves in having amongst us this evening one of our distinguished Associates. Prof. Langley is known to all of us as the director of one of the principal American observatories. He has done most important work at the Allegheny Observatory, and I think we might ask him to tell us something of the work he has been engaged upon.

Prof. Langley. Mr. President, I did not anticipate that I should have had this honour done me to-night, and I am quite unprepared to make any formal presentation of the work of the observatory you have so kindly alluded to. I can only say that for the last four or five years particularly we have been engaged in studies connected with the energy which comes to us from the Sun, regarded not merely as light, but as heat; and in connection with that we have been studying the obstacles which our atmosphere offers to the transmission of those radiations. Some five or six years ago we began to study more particularly in the solar spectrum those regions in the infra-red which lie beyond the scope of the photographic operations which Captain Abney has so successfully carried on, and we had pushed our observations to a point which I think had hardly been reached by others, when we found the hindrances due to the atmosphere prevented us from going further with the means we then possessed, which consisted largely of such instruments as the thermopile. We were then occupied for a considerable time in perfecting an apparatus more accurate than the thermopile to carry those investigations further. Now we have an instrument which I have called the bolometer, which consists of an extremely fine wire or metallic tape in which an electric current passes and on which the heat falling from any heavenly body causes a change in the conductivity and a consequent deflection in the galvanometer. We commenced mapping the spectrum in the infra-red, using for that purpose a grating which should give us a spectrum of the wave-length scale, the prism giving us, as is well known, a spectrum which differs with every instrument employed. We know of the excellent work which the prism does, and is doing, but for the point immediately in hand, the grating seems preferable. The heat, however, in the grating spectrum is in certain portions about $\frac{1}{400}$ of what it is in like portions of the prismatic spectrum, and an instrument of corresponding delicacy was required to measure in the infra-red. Another object we had in view was to find what obstacle our atmosphere opposed to the radiation of the solar and lunar energy and that of other celestial bodies, which could only be done, as it seemed to us, by studying such narrow spaces as could be treated like individual rays, which this instrument, consisting of a platinum thread, in some cases less than one-thousandth of an inch in diameter, was perfectly well able to do. Without detaining you with a longer account of the preliminary work, I may say it gave us finally an instrument which could measure amounts of heat in excessively minute spaces, and make explorations which gave us promise of ascertaining more about the effect of the atmosphere than had been done before. In the course of using these instruments we fitted out from the Observatory, with the aid of a citizen of Pittsburg and of the United States Government, an expedition to ascend one of the highest mountains in its territories; this mountain, some 15,000 feet in height, is in the Sierra Nevada, and to that spot we carried the elaborate accessory apparatus. We pursued our work upon the mountain, and there discovered, I think, regions of the spectrum before undiscriminated, and continued our work after our descent. We have been occupied lately in the same way, and have carried first our exploration in this invisible spectrum to a wave-length of about 28,000 on Angstrom's scale. That may perhaps have a more definite meaning to some gentlemen present when I mention that the extreme visible wave-length in the spectrum is about 7000 to 7500, and the portion covered by the most interesting and valuable researches of Capt. Abney extends to 11,000 or 12,000. We have been down, then, to about 27,000 on the Sun, while more lately, in carrying our researches into the radiation from the Moon, we have found it necessary to measure the wave-lengths given out at very low temperatures, and have incidentally measured wave-lengths from artificial sources as low as 80,000. But at that point we found that no ray from the Sun came through the Earth's atmosphere, which seems to leave us only one of two conclusions—either that the Earth's atmosphere is impervious to such solar radiations, or else one we are not disposed at first sight to accept, that the solar radiation does not embrace any such wave-lengths within it. We have gone on with observations of the kind which I am detailing, and which I fear lie too far from the field of the regular astronomer to interest you, but which, to the student of what has been somewhat ironically called "inexact" astronomy, possesses a general interest. We have more recently been engaged in measuring anew the energy of the Sun, obtaining a larger value of the solar constant. In studying these homogeneous rays, we have found the atmospheric absorption to be much larger than hitherto supposed, the result leading us to think it is nearly double that obtained by former methods—a difference which seems at first sight irreconcilable with the conclusions of exact experimenters who have already worked in that field, but which really implies no impugnment of the accuracy of the experiments, but only of their deductions from them. We are pursuing these researches, which all deal with the energy which comes to us from heavenly bodies in the form of heat; and I may mention as an illustration of the delicacy of the apparatus we are now using, that whereas former most skilful observers have found it difficult to detect any in the Moon, we find ourselves able to get an amount of lunar heat with a small mirror of 8 inches diameter that will drive the needle many times through the length of the scale of the galvanometer, so that we are obliged to damp it in order to control it. We find ourselves able to take that heat through a train of rock-salt prisms and lenses and draw it out into a heat spectrum and measure any distinct portion of it, and investigations of that kind and others intended to utilize the effects of this solar energy are still going on. All these have to me an interest that they tend to broad utilities, and I hope that they may lead us to something of general use, as we are looking to such broad problems as the utilization of solar heat and other matters which in their practical concern are widely apart from the old fields of astronomy.

In the course of the preceding work I may be allowed to say that in ascending to this height of from 12,000 to 15,000 feet I have been lately struck with the immense advantages that such altitudes afford, not only to the observer in physical astronomy, but in other fields, especially the exceedingly good definition for double-star observations, the admirable clearness and transparency in the atmosphere for the purpose of photographing the heavenly bodies, and the wonderful appearance of the nebulæ at such heights; and I have succeeded, through the representations of the War Department, in obtaining a Government reservation of a tract of 100 square miles in the Sierra Nevada Mountains for the possible erection of a physical observatory, though I fear there is no immediate prospect of its being built. Still I think that with the now successful experiments of the Lick Observatory at the lower elevation of between 4000 to 5000 feet there is a prospect, I hope not too far distant, of a possible realization of such a physical observatory on Mount Whitney, for we are approaching to those conditions foreshadowed so long ago as the time of Sir Isaac Newton, and find that to pursue our observations to the best advantage we must get above this turbid atmosphere in which we are working under difficulties which we must ultimately triumph over, difficulties every observer is now aware of in every field of work. Allow me, in closing, to thank you for the kind way you have listened to these very unpremeditated remarks.

Mr. Stone. Some years ago I found in this matter that the difficulty I experienced was not the want of sensitiveness in my instrument, but rather, in one respect, an over-sensitiveness. I found, in the determinations I was making, it was perfectly hopeless to do anything in the way of quantitative results on account of radiation. So long as I made use of one delicate thermopile for observations, I found I was simply registering every variation of the absorption of the atmosphere, as well as the heat I was receiving, and I found it necessary for this experiment to use two opposed thermopiles, and then by using differential results I found I could get quantitative results with some degree of accuracy. I should be glad to know whether Professor Langley in his work with an instrument of greater sensitiveness than that I was using has not experienced this same kind of difficulty in an increased degree.

Mr. Common. I should like to ask Mr. Langley to give us, if possible, a little more information as to the construction of the bolometer. Is it platinum deposited on glass, or is it wire of platinum spread out?

Prof. Langley. In answer to Mr. Common I would say that we made the first from iron which had been made in a contest between the American and English iron masters, and of which 15,000 sheets laid one on another made up one inch. From that thin iron we cut our first strips; but afterwards we found ourselves driven to the use of platinum, since the iron was good while it lasted, but got rusty. The bolometer is now made with strips of platinum which are from $\frac{1}{25000}$ to $\frac{1}{50000}$ of an inch thick. The width of the strip is usually from $\frac{1}{120}$ to $\frac{1}{250}$ of an inch. When it is used for measuring the spectrum, this or a still narrower wire is passed down parallel to the Fraunhofer lines, and as it touches each spectral line where the eye would see a blackness the wire feels the cold, and there is a corresponding change of resistance, and equally so whether the eye sees it or not. We have it now in the form of a wire like a reticule, and controlled by a micrometer-screw. We are using it also, in a modified form, in observations of the Moon, in which it is stretched backwards and forwards several times so as to occupy a certain amount of area. In answer to Mr. Stone, I may say that we have experienced the difficulty which he has alluded to—that is, we found that variations of about 8 to 10 per cent. in an entirely clear sky constantly presented themselves (I mean that clouds constantly passed which the eye did not recognize, but the instrument did); and a large portion of the variations which have been referred to we found we could eliminate as due to local causes; and by putting the instrument at the bottom of a tube and by the use of numerous diaphragms we entirely shut out the local air-currents, which are the cause of a good deal of trouble ascribed to external atmospheric variation.

Mr. Stone. Might you not adopt the same expedient I did, and have two reverse bolometers?

Mr. Langley. We have been to a great deal of labour in that. There are certain technical difficulties peculiar to this form of instrument; but we have finally been led to try it in the form I have indicated.

The President. We have all been delighted to hear the remarks of Professor Langley. I can assure him that his researches are not strange to us. We have for many years had our eyes directed towards the Allegheny Observatory; and he has sent many papers to this country in explanation of his various researches. I have read them with the greatest profit and the greatest pleasure; and after hearing his remarks to-night, I am confident that all present will leave this room feeling they have learned something from them. I will ask you to return thanks to him by acclamation. (Applause.)

Mr. Common exhibited some enlargements of photographs of the nebula of Orion taken in 1883. The enlargements had been made by Mr. Lockyer. He called attention to the fact that the enlargements had been made by commencing with the photographs that had had the shortest exposure, with the view to make out the differing contour of the lines indicated in the nebula. The time of exposure varied from a few minutes to 60 minutes. Each duration produced a varying amount of detail, the longer exposures bringing out fresh details in some parts, but obscuring others. What an exposure of 160 minutes would produce he did not know; but he believed that while some portions would be lost, others would come out that were at present invisible. The power of photography was of such a character that it would show on paper or glass what the eye would never be able with any instrument to appreciate. Stars which had never yet been seen would be brought out by photography: and he was anxious to see some ardent student of photography step to the front and take the task in hand.

Mr. Knobel read a paper from *Lord Crawford* on an observation of Jupiter made on Feb. 18, 1885, at Dunecht by Dr. Copeland, and which exhibited the shadow of Satellite I. on the planet. The transit of Satellite II. next day was attended by a partial concealment of its shadow. Mr. Knobel remarked that there was no novelty in the observation recorded. In 1872 he had sketched a satellite as concealing half of its shadow in a way very similar to one of the sketches accompanying Lord Crawford's paper.

Captain Noble explained a paper on the occultation of B.A.C. 3836. The star faded out in a way which suggested the idea of an objective disk instead of a diffraction-disk. They had heard of double stars too close to be separated by any optical power being discovered by their behaviour when occulted; and he only brought forward this observation in order that the star might be observed by big telescopes to see whether it were double.

Mr. Chambers. May I ask what is the result of the poll which was to have been taken as to the time of meeting?

The President. You will find the result posted up; but I will read it. The votes in favour of holding the Meeting at 8 o'clock are 104 Fellows; in favour of 5 o'clock 125 Fellows; and neutral 149 Fellows. For other hours there are 16 votes; and 100 Fellows have not replied.

Mr. Chambers. What is to be the practical result?

The President. Nothing is decided at present.

The following papers were announced:—

Lieut.-Col. Tupman. "Occultations of Stars by the Moon in the Years 1876–80, and resulting Final Equations between the Errors of the Tables and the Errors of Observation."

Dr. R. Copeland. "Occultation of Aldebaran, 1885, Feb. 22, observed at Dun Echt."

The following gentlemen were elected Fellows of the Society:—Lieut. S. G. Burrard, R.E.; J. H. Honeyburne, M.A.; J. M'Kerrow, Surveyor-Gen. of New Zealand; J. A. Westwood Oliver; and Major H. J. Watson.

The meeting adjourned at 9.45 P.M.

Liverpool Astronomical Society.

THE seventh Meeting of the Session was held on the 13th instant, the Rev. T. E. Espin, B.A. (President), in the chair. The Rev. J. H. Honeyburne, M.A., F.R.A.S., read a paper on "Evenings in Town with a small Telescope." He said it was generally supposed that astronomical observation required an exceptionally good position, besides other conveniences; but he had found a $3\frac{1}{4}$-in. telescope, in a little back yard in Liverpool, quite sufficient to render the science a most delightful pursuit. He had noticed that members generally went in for a study and comparison of star magnitudes, or for an elaboration of mysterious hieroglyphics supposed to represent the bottom of some ring-plain on the Moon. No doubt this was all very important and interesting to the initiated; but he made no claim to originality, and would merely give an outline of the colours of some stars, and the closeness or faintness of others. The tints of many of the stars were most lovely (β Cygni and δ Cephei he gave as examples); whilst the orange and blue of γ Persei fully entitled it to rank amongst the flowers of the sky.

Mr. T. G. Elger, F.R.A.S., hoped the important communication from Dr. Klein, read at the last Meeting, would direct the attention of selenographers to the curious dark markings south of Copernicus. Since the publication of Schmidt's work, he had carefully scrutinized this region, though with not much result, except that on Nov. 3 last he had glimpsed two very minute black spots. These dusky features seemed to suggest an analogy to Mount Rossi on the southern flank of Etna, which, according to Scrope, completely covered the surrounding country with a deposit of black sand during an eruption in 1669. Possibly something of this kind might now be occurring on the Moon; and certainly it would be amongst the smaller craters that we should look for evidence of present volcanic activity.

The President said he had, at Dr. Spitta's request, lately tested his new occulting eyepiece. It might briefly be described as a Ramsden eyepiece fitted with two pairs of movable shutters at the focus. Its work on double stars was excellent, especially when the companion happened to be faint and not too close, and he had by its aid discovered a new companion to 27 Hydræ. This was a wide and lovely pair *s.p.* *α* Hydræ, and was noted by the elder Herschel, though he did not think he had discovered its triplicity. The pair was of 6·0 and 7·0 mag.; but on examining them with this eyepiece, a small comes of 11 mag. (Struve's scale) was at once detected about 8″ from the 7·0 mag. star and at about 190°. Altogether, he thought the eyepiece supplied a want long felt, but, from the nature of the eyepiece, there was an unfortunate liability to "ghosts:" hence there was a danger of finding companions which had no existence.

Mr. J. E. Gore, F.R.A.S., contributed a series of observations of the remarkable variable *Mira* Ceti. According to Argelander's formula, the maximum was due on Jan. 28, but, from his own observations, this was not attained until Feb. 4, when he estimated it equal to *α* Ceti, or about 2·7 mag. The increase of light was at first very rapid, but it became afterwards much slower, and seemed to remain constant from Feb. 4 to Feb. 13.

Mr. R. Wilding, Hon. Curator of Preston Observatory, announced his discovery of Tempel's Comet. Its position on April 8 was R.A. 12^h and about 19° N. dec. The nucleus appeared like a 13th mag. star surrounded by a nebulous disk.

The Secretary gave notice that he would bring forward for discussion at the next Meeting a motion to enable Members who reside at a distance to vote at the Annual General Meeting by proxy.

Eleven gentlemen were elected members; and Dr. Copeland, of Dun Echt, was elected an Associate of the Society.

The Solar Corona.

[By the kind permission of Dr. Huggins, we are here able to reproduce the latter part of his lecture on "The Solar Corona," delivered at the Royal Institution on February 20, 1885. In the earlier portion of his lecture, Dr. Huggins explained his method for photographing the corona when the Sun was not eclipsed, and described the results he had obtained. These have already been referred to in the 'Observatory,' Vol. vi. pp. 26, 342, and Vol. vii. p. 376. The substance of the lecture as a whole has appeared in an article in the 'Nineteenth Century' for April 1885.]

Six hypotheses have been suggested as to the probable nature of the corona:—

1. That the corona consists of a gaseous atmosphere resting upon the Sun's surface and carried round with it.

2. That the corona is made up, wholly or in part, of gaseous

and finely divided matter which has been ejected from the Sun, and is in motion about the Sun from the forces of ejection, of the Sun's rotation, and of gravity,—and possibly of a repulsion of some kind.

3. That the corona resembles the rings of Saturn, and consists of swarms of meteoric particles revolving with sufficient velocity to prevent their falling into the Sun.

4. That the corona is the appearance presented to us by the unceasing falling into the Sun of meteoric matter and the débris of comets' tails.

5. That the coronal rays and streamers are, at least in part, meteoric streams strongly illuminated by their near approach to the Sun, neither revolving about nor falling into the Sun, but permanent in position and varying only in richness of meteoric matter, which are parts of eccentric comet-orbits. This view has been supported by Mr. Proctor, on the ground that there must be such streams crowding richly together in the Sun's neighbourhood.

6. The view of the corona suggested by Sir William Siemens in his solar theory.

It has been suggested, even, that the corona is so complex a phenomenon that there may be an element of truth in every one of these hypotheses. Any way this enumeration of hypotheses more or less mutually destructive shows how great is the difficulty of explaining the appearances which present themselves at a total solar eclipse, and how little we really know about the corona.

An American philosopher, Professor Hastings, has revived a prior and altogether revolutionary question: Has the corona an objective existence? Is it anything more than an optical appearance depending upon diffraction? Professor Hastings has based his revival of this long discarded negative theory upon the behaviour of a coronal line which he saw, in his spectroscope, change in length east and west of the Sun during the progress of the eclipse at Caroline Island. His view appears to rest on the negative foundation that Fresnel's theory of diffraction may not apply in the case of a total eclipse, and that at such great distances there is a possibility that the interior of the shadow might not be entirely dark, and so to an observer might cause the appearance of a bright fringe around the Moon*.

Not to speak of the recent evidence of the reality of the corona from the photographs which have been taken when there is no intervening Moon to produce diffraction, there is the adverse evidence afforded by the peculiar spectra of different parts of the corona and by the complicated and distinctly peculiar structure seen in the photographs taken at eclipses. The crucial test of this theory appears to be, that if it be true, then the corona would be much wider on the side where the Sun's limb is least deeply covered, that is to say the corona would alter in width on the two sides during the progress of the eclipse. Not to refer to former eclipses where photographs taken at different times and even at different places have been found to agree, the photographs taken during the eclipse at Caroline Island show no such changes. M. Janssen says, "Les formes de la couronne ont été absolument fixes pendant toute la durée de la totalité." The photographs taken by Messrs. Lawrance and Woods also go to show that the corona suffered no such alterations in width or form as would be required by Professor Hastings' theory during the passage of the Moon.

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* "Report of the Eclipse Expedition to Caroline Island, May 1883," Memoirs of the National Academy of Sciences, Washington.

We have therefore, I venture to think, a right to believe in an objective reality of some sort about the Sun corresponding to the appearance which the corona presents to us. At the same time some very small part of what we see must be due to a scattering of the coronal light itself by our air, but the amount of this scattered light over the corona must be less than what is seen over the dark Moon.

That the Sun is surrounded by a true gaseous atmosphere of relatively limited extent there can be little doubt, but many considerations forbid us to think of an atmosphere which rises to a height which can afford any explanation of the corona, which streams several hundred thousand miles above the photosphere. For example, a gas at that height, if hundreds or even thousands of times lighter than hydrogen, would have more than metallic density near the Sun's surface, a state of things which spectroscopic and other observations show is not the case. The corona does not exhibit the rapid condensation towards the Sun's limb which such an atmosphere would present, especially when we take into account the effect of perspective in increasing the apparent brightness of the lower regions of the corona. There is, too, the circumstance that comets have passed through the upper part of the corona without being burnt up or even sensibly losing velocity.

There can scarcely be doubt that matter is present about the Sun wherever the corona extends, and further that this matter is in the form of a fog. But there are fogs and fogs. The air we breathe, when apparently pure, stands revealed as a dense swarming of millions of motes if a sunbeam passes through it. Even such a fog is out of the question. If we conceive of a fog so attenuated that there is only one minute liquid or solid particle in every cubic mile, we should still have matter enough, in all probability, to form a corona. That the coronal matter is of the nature of a fog is shown by the three kinds of light which the corona sends to us—reflected solar light scattered by particles of matter solid or liquid, and, secondly, light giving a continuous spectrum, which tells us that these solid or liquid particles are incandescent, while the third form of spectrum of bright lines, fainter and varying greatly at different parts of the corona and at different eclipses, shows the presence also of light-emitting gas. This gas existing between the particles need not necessarily form a true solar atmosphere, which the considerations already mentioned make an almost impossible supposition, for we may well regard this thin gas as carried up with the particles, or even to some extent to be furnished by them under the Sun's heat.

It will be better to consider first the probable origin of this coronal matter, and by what means it can find itself at such enormous heights above the Sun.

There is another celestial phenomenon, very unlike the corona at first sight, which may furnish us possibly with some clue to its true nature. The head of a large comet presents us with luminous streamers and rifts and curved rays, which are not so very unlike, on a small scale, some of the appearances which are peculiarly characteristic of the corona *. We do not know for certain the conditions under which these cometary appearances take place, but the hypothesis which seems on the way to become generally accepted attributes them to electrical disturbances, and especially to a repulsive force acting from the Sun, possibly electrical, which varies as the surface and not, like gravity, as the mass. A force of this nature in the case of highly attenuated matter can easily master the force of gravity, and, as we see in the tails of comets, blow away this thin kind of matter to enormous distances in the very teeth of gravity.

If such a force of expulsion is experienced in comets, it may well be that it is also present in the Sun's surroundings. If this force be electrical it can only come into play when the Sun and the matter subjected to it have electric potentials of the same kind, otherwise the attraction on one side of a particle would equal the repulsion on the other. On this theory, the coronal matter and the Sun's surface must both be in the same electrical state—the repelled matter negative if the Sun is negative, positive if the Sun is positive.

The grandest terrestrial displays of electrical disturbance, as seen in lightning and the aurora, must be of a small order of magnitude as compared with the electrical changes taking place in connection with the ceaseless and fearful activity of the Sun's surface; but we do not know how far these actions, or the majority of them, may be in the same electrical direction, or what other conditions there may be, so as to cause the Sun's surface to maintain a high electrical state, whether positive or negative. A permanence of electric potential of the same kind would seem to be required by the phenomena of comets' tails.

If such a state of high electric potential at the photosphere be granted as is required to give rise to the repulsive force which the phenomena of comets appear to indicate, then considering the gaseous irruptions and fiery storms of more than Titanic proportions which are going on without ceasing at the solar surface, it does not go beyond what might well be, to suppose that portions of matter ejected to great heights above the photosphere and often with velocities not far removed from that which would be

* See "Comets," Royal Institution Proceedings, Vol. x. p. 1.

necessary to set it free from the Sun's attraction, and very probably in the same electric state as the photosphere, might so come under this assumed electric repulsion as to be blown upwards and to take on forms such as those seen in the corona: the greatest distances to which the coronal streamers have been traced are small as compared with the extent of the tails of comets, but then the force of gravity which the electrical repulsion would have to overcome near the Sun would be enormously greater.

It is in harmony with this view of things that the positions of greatest coronal extension usually correspond with the spot zones where the solar activity is most fervent; and also that a careful examination of the structure of the corona suggests strongly that the forces to which this complex and varying structure is due have their seat in the Sun. Matter repelled upwards would rise with the smaller rotational velocity of the photosphere, and lagging behind would give rise to curved forms; besides, the forces of irruption and subsequent electrical repulsion might well vary in direction and not be always strictly radial, and under such circumstances a structure of the character which the corona presents might well result. The sub-permanency of any great characteristic coronal forms, as, for example, the great rift seen in the photographs of the Caroline Island eclipse and also in those taken in England a month before the eclipse and about a month afterwards, must probably be explained by the maintenance for some time of the conditions upon which the forms depend, and not to an unaltered identity of the coronal matter; the permanency belonging to the form only, and not to the matter, as in the case of a cloud over a mountain top, or of a flame over the mouth of a volcano. If the forces to which the corona is due have their seat in the Sun, the corona would probably rotate with it; but if the corona is produced by conditions external to the Sun, then the corona might not be carried round with the Sun.

We have seen that the corona consists probably of a sort of incandescent fog, which at the same time scatters to us the photospheric light. Now we must bear in mind the very different behaviour of a gas, and of liquid or solid particles in the near neighbourhood of the Sun. A gas need not be greatly heated, even when near the Sun, by the radiated energy; heated gas from the photosphere would rapidly lose heat; but, on the other hand, liquid or solid particles, whether originally carried up as such, or subsequently formed by condensation, would absorb the Sun's heat, and at coronal distances would soon rise to a temperature not very greatly inferior to that of the photosphere. The gas which the spectroscope shows to exist along with the incandescent particles of the coronal stuff may therefore have been carried up as gas, or have been in part distilled from the coronal particles under the enormous radiation to which they are exposed. Such a view would not be out of harmony with the very different heights to which different bright lines may be traced at different parts of the corona and at different eclipses. For obvious reasons, gases of different vapour-density would be differently acted upon by a repulsive force which varies as the surface and would be to some extent winnowed from each other; the lighter the gas the more completely would it come under the sway of repulsion, and so would be carried to a greater height than the gas more strongly held down by gravity. The relative proportions, at different heights of the corona, of the gases which the spectroscope shows to exist there (and recently Captain Abney and Professor Schuster have shown that in addition to the bright lines already known, the spectrum of the corona of 1882 gave the rhythmical group of the ultra-violet lines of hydrogen which are characteristic of the photographic spectra of the white stars, and some other lines also) would vary from time to time, and depend in part upon the varying state of activity of the photosphere, and so probably establish a connection with the spectra of the prominences. This view of the corona would bring it within the charmed circle of interaction which seems to obtain among the phenomena of sun-spots and terrestrial magnetic disturbances and auroræ.

Many questions remain unconsidered; among others, whether the light emitted by the gaseous part of the corona is due directly to the Sun's heat, or to electrical discharges taking place in it of the nature of the aurora. Further, what becomes of the coronal matter on the theory which has been suggested? Is it permanently carried away from the Sun, as the matter of the tails of comets is lost to them? Among other considerations it may be mentioned that electric repulsion can maintain its sway only so long as the repelled particle remains in the same electrical state; if through electric discharges it ceases to maintain the electrical potential it possessed, the repulsion has no more power over it, and gravity will be no longer mastered. If, when this takes place, the particle is not moving away with a velocity sufficiently great to carry it from the Sun, the particle will return to the Sun. Of course, if the effect of any electric discharges or other conditions has been to change the potential of the particle from positive to negative, or the reverse, as the case may be, then the repulsion would be changed into an attraction acting in the same direction as gravity. In Mr. Wesley's drawings of the corona, especially in those of the eclipse of 1871, the longer rays or streamers appear not to end, but to be lost in increasing faintness and diffusion, but certain of the shorter rays are seen to turn round and to descend to the Sun *.

It is difficult for us living in dense air to conceive of the state of attenuation probably present in the outer parts af the corona. Mr. Johnstone Stoney has calculated that more than twenty figures

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* For a history of opinion of the nature of the Corona, see papers by Prof. Norton, Prof. Young, and Prof. Langley in the 'American Journal of Science'; also 'The Sun,' by Prof. Young; and 'The Sun the Ruler of the Planetary System,' and various Essays, by Mr. R. A. Proctor.

are needed to express the number of molecules in a cubic centimetre of ordinary air, and Mr. Crookes shows us in his tubes that matter, even when reduced to one-millionth part of the density of ordinary air, can become luminous under electrical excitement. [A glass bulb about 4 inches in diameter, kindly lent to me by Mr. Crookes, was exhibited, in which a metal ball about half an inch in diameter formed the negative pole. Under a suitable condition of the induction current, this ball was seen to be surrounded by a corona of bluish-grey light which was sufficiently bright to be seen from all parts of the theatre.] It is probable that these tubes must be looked upon as crowded cities of molecules as compared with the sparse molecular population of the great coronal wastes.

I forbear to speculate further, as we may expect more information as to the state of things in the corona from the daily photographs which will be shortly commenced at the Cape of Good Hope by Mr. Ray Woods under the direction of Dr. Gill.

William Huggins.

Jupiter's first Satellite occulting its Shadow.—Notes on the Transits and Nomenclature of Jupiter's Satellites.

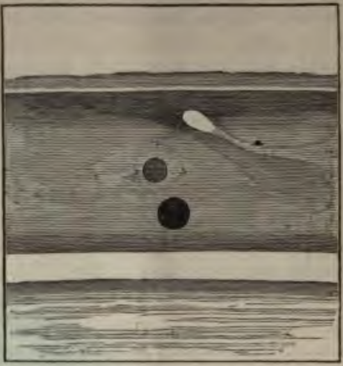
On February 18, 1885, about 11^h 30^m, I observed Jupiter with power 252 (single lens) on my 10-inch reflector. According to the 'Nautical Almanac' the shadow of Sat. I. and the satellite itself were in transit, the times of ingress and egress being very nearly identical owing to the planet arriving at opposition a few hours later (Feb. 18, 20^h).

I could not see the satellite, though it is usually visible anywhere near mid-transit as a faint grey spot, and may be followed well whenever the seeing is good. I, however, found what I conceive to have been the shadow under the form of a black crescent or bow-shaped spot, situated between the W. limb and centre, and evidently obtruding from the W. side of the satellite, which must have overlapped all the E. region of the shadow.

Lord Crawford referred to this interesting little phenomenon at the last meeting of the R.A.S. ('Observatory,' April, p. 109) as observed at Dun Echt; but I do not know whether Dr. Copeland regarded the occultation of the shadow as complete, or whether he, similarly to myself, saw the arc of shadow immediately preceding the satellite across the disk.

During the present apparition of Jupiter I have observed several transits of Sats. III. and IV. and their shadows. When well on the disk, III. invariably becomes a dark brown spot, though obviously much lighter than its shadow. IV. exhibits even a deeper tone than III., and though not quite black is little less so than its shadow, from which it is sometimes only distinguished with difficulty.

On December 31, 1884, at 19^h 20^m I saw, between the tw equatoreal dark belts and grouped near the central meridian o Jupiter, a trio of interesting objects including the equatoreal whit spot, then very brilliant, and the shadows of Sats. II. and III. Wha



Equatoreal White Spot and Shadows of Satellites II. and III. near Central Meridian, 1884, Dec. 31, 19^h 20^m.

struck me at once was the very marked difference in the relative tints of the two shadows. That of III. immediately S. of the great N. belt looked quite black, but II., on the immediate *s.p.* side of III., was a pale chocolate colour. The shadow of II. was projected on a somewhat lighter background, possibly in some degree self-luminous, but I believe the shadow of II. is invariably far lighter than that of any other satellite. Sat. II. is only visible in transit as a bright spot near the limbs, hence its reflective power is considerably greater than I., which also much exceeds III. and IV. Is this satellite involved in an atmosphere of considerable density occasioning undue luminosity relatively to the others? If so, the feeble shadow it transmits to Jupiter may be in part explained by the effects of refraction.

While writing, I may also ask is there any tangible reason why the mythological names originally applied to the satellites by Simon Marius should any longer be withheld from use? Such names have been given to the satellites of the other planets, and are far preferable to the Roman numerals affixed to the moons of Jupiter. Moreover, the latter style is a departure from an otherwise uniform method. When the satellites of Mars were discovered, one of the very first things discussed was the question as to suitable names. Certainly no one dreamed of permanently calling them Satellites I. and II. It seems that the titles Io, Europa, Ganymede, and Callisto were not accepted by astronomers, as Simon Marius claimed priority to Galileo in discovering the satellites of Jupiter, and the recognition of his names would be supposed to advance his pretensions. But such objections have long since become obsolete, and there seems no longer any ground for rejecting the specific titles assigned by Marius, who was one of the very first to observe the satellites. If there continues to be any objection to the mythological names he selected, let us adopt others. We shall soon become familiar with the change, and the innovation is likely to cause very little confusion, while it will be far more satisfactory for reference, and introduce perfect uniformity in the system of satellite nomenclature.

The relative aspects of the satellites of Jupiter and their shadows in traversing the light and dark zones of the surface supply several interesting points for discussion, but what now appears desirable is a critical review of these phenomena during several oppositions. No doubt can be expressed that a thoroughly practical research of this sort would lead us to the explanation of the peculiarities so often described. Past records are too broken, incomplete, and varied to possess consistent accuracy, for it is almost invariably found that, in work which is the outcome of many independent authorities, it is difficult to deal with the *apparent* discordances which are sure to make themselves felt in any attempted collation of the materials. A single observer with a good eye, an adequate telescope, a natural aptitude for the work, and a ready perception of the salient points calling for elucidation, might soon obtain many details of interest in reference to these transits, which always form a great attraction to amateurs.

Bristol, 1885, Mar. 29.

W. F. DENNING.

Comet II. 1884 (*Barnard*).

PROF. H. V. EGBERT of the Dudley Observatory has computed the following elliptic elements of the above comet from four normal places:—

Elliptic Elements.

T	1884, Aug. 16·482833, G.M.T.	
ω	301° 2′ 42″·4	Mean Eq. 1884·0.
☊	5 8 37 ·5	
i	5 27 33 ·2	
φ	35 43 44 ·8	
log q	0·1071047	
log a	0·4879630	
μ	657″·752	
P	1970·345 days.	

These elements were computed from the following four places:—

	G.M.T.	α 1884·0.	δ 1884·0.
1884.	h m s	h m s	° ′ ″
July 23	23 35 12	16 4 38·73	−37 14 25·9
Aug. 22	0 3 10	17 39 9·12	−35 37 48·8
Sept. 17	8 32 12	19 27 51·05	−28 58 5·2
Oct. 24	6 51 18	21 1 56·98	−19 17 46·5

These places were obtained thus:—

1st and 2nd are each the combination of two Windsor (N.S.W. observations, which were in both cases checked by Cape of Goo Hope observations.

3rd place is the mean of observations at Cape of Good Hope, Arcetri, and Nice.

4th place mean of two observations at Arcetri, one at Vienna, and one at Nice.

The following are the residuals (C−O) from comparisons of the elements with the above places.

(C−O).

	I.	II.	III.	IV.
	″	″	″	″
Δλ........	0·2	0·0	0·0	0·1
Δβ	0·0	+1·4	+4·2	0·0

The care with which the places have been selected and the computations made justifies the belief that the orbit will be very accurate.

Vanderbilt University Observatory,
Nashville, Tennessee, U.S.A.

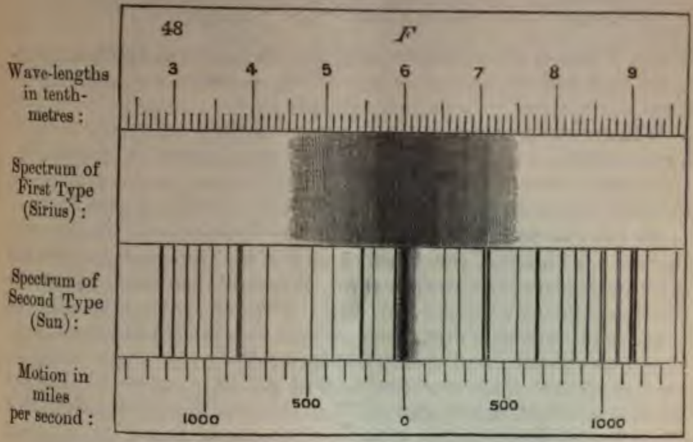
E. E. Barnard.

The Motions of Stars in the Line of Sight.

[Continued from p. 122.]

The proper adjustment of the spectroscope, though it may require a lengthened explanation to describe, is easy to effect, and the real difficulty of the work now lies in the character of the stellar spectra themselves. No two of these are precisely alike, but they may be grouped into two great classes—the blue stars, with spectra crossed only by a few broad ill-defined bands due to hydrogen, and the yellow stars, with many hundreds of fine lines. In observing stars of the first class there is the difficulty, in some cases almost the impossibility, of determining exactly where the centre of the broad ill-defined band is. As many of our brightest stars, Sirius, Vega, Procyon, belong to this class, a high dispersion can often be employed, but no advantage is thereby secured, since the band is made broader in just the same proportion as the dispersion is increased, and whether the dispersion be high or low, the uncertainty of the bisection is great as compared with any admissible displacement due to the motion of the star. Fig. 2 will render this more evident than any mere description could do. The spectrum selected, that of Sirius, is undoubtedly an extreme case, few stars showing quite so broad a line, but many have lines which are quite as ill-defined in character. Spectra of the second class are much more satisfactory to measure; the lines are narrow and sharp, and if they can be seen and held steadily there is no possibility of uncertainty about their bisection. But unfortunately this condition is not easy to secure, and it is as difficult, on an unsteady night, to detect the fine lines in a stellar spectrum as to see the "canals" on Mars or the finer markings on Jupiter. Each

Fig. 2.


48
F
Wave-lengths in tenth-metres :
3
4
5
6
7
8
9
Spectrum of First Type (Sirius) :
Spectrum of Second Type (Sun) :
Motion in miles per second :
1000
500
0
500
1000

Region near F in Spectra of the First and Second Types.

type of spectrum, therefore, presents its own difficulties. The broad lines of the first are easy indeed to see, but difficult to bisect; the fine lines of the second are hard to see, but when seen can be very exactly measured.

The importance of good definition is shown by the difficulty which is experienced in the observation of low stars. Fomalhaut has never been satisfactorily observed at Greenwich; and the observations of Sirius and of most of the stars in Orion are rougher than those of the stars in Cygnus and Cepheus, though the former are on the average the brighter. The difficulty is especially felt with stars of the second type, since, if the definition be poor, the line under observation may be confused with others near it; thus nothing would be easier, in a faint solar spectrum badly seen, than to confuse the F line with the line at λ 4859 (see fig. 2), and to regard the two as forming but a single line. This would give the effect of a displacement towards the blue, and since it would be due to the character of the spectrum itself, it could not be avoided by any care in adjusting the spectroscope. Since, however, spectra of this type generally offer several lines suitable for measurement, it may be safely assumed that no such source of error exists for stars for which several different lines have given the same result. The following table shows the motions obtained for the five brightest stars of the second type :—

	Miles per second.		Nights.	
Star.	b_1.	F.	b_1.	F.
Aldebaran	+21	+28	17	18
Capella	+31	+22	12	19
Pollux	−25	−42	17	14
Arcturus	−47	−44	21	26
α Cygni	−33	−37	11	22

The F line in the spectrum of Pollux is faint and difficult to see. In this and in the following tables, + denotes a motion away from our system, − a motion towards it.

Where the spectrum is too faint, or its lines too feeble for such a comparison, only the employment of more powerful instruments and under more favourable conditions of atmosphere, can assure us that our observations have not been affected by this source of error.

As a check on the adjustment of the spectroscope, the lines in the spectra either of the Moon or of the sky are compared with the lines in the comparison-spectrum. It is true that neither the Moon nor the diffused daylight from the sky is observed under quite the same conditions as a star, since in both cases the light much more than fills the slit; still if care has been taken to have the slit narrow enough to cut off some of the light of the star, and it is found that no displacement can be detected between the lines of the Moon and the comparison-spectrum, it is fair to assume that a displacement observed in the spectrum of a star is real, and not fictitious.

On a few occasions Venus and Mars have been observed as a test of the accuracy of the star-measures. On every occasion the displacement observed has been in the right direction, but usually slightly, or, in the case of Venus, considerably too large in amount. The difficulty of measuring accurately a very small distance probably partly accounts for this discordance.

Perhaps the most satisfactory proof that the observations as a whole were affected by no serious instrumental error would be given by the observation of a change in the amount of displacement noticed in a star-line corresponding to the season of the year at which the star was observed. For the Earth, in its annual revolution round the Sun, is at one time approaching and at another receding from any particular star; and the measures of displacement must be cleared of the effect of this motion before the true motion of the star towards the solar system can be deduced. Unfortunately the observations have, as a rule, been made between sunset and midnight, and on stars near the meridian. The Earth was therefore usually approaching the star observed; and the measures made when it was receding have been few in number, and have frequently been obtained under disadvantageous circumstances. There is therefore but scanty material, and that not very satisfactory, for ascertaining if the observations show the effect of the Earth's motion as they should. Such material as there is seems, however, to indicate that there is a decided tendency to exaggerate the amount of displacement when the motion of the star is partly neutralized by that of the Earth, and the amount to be measured therefore small.

The following table gives the mean *observed* motions, uncorrected for the Earth's motion, for several stars which have been observed sufficiently frequently in opposite parts of the Earth's orbit:—

Star.	Earth and star moving the same way.		Earth and star moving different ways.		Difference in observed motion of star.	Difference in Earth's motion.
	Nights.	Star's motion.	Nights.	Star's motion.		
ldebaran ...	25	+35·2	10	+25·5	+ 9·7	+21·7
Lyræ	12	−34·4	44	−32·3	− 2·1	− 9·4
Aquilæ......	11	−17·4	28	−14·6	− 2·8	−13·6
Cygni	8	−20·2	6	− 5·9	−14·3	−11·7
Cygni	13	−34·0	19	−31·6	− 2·4	−11·4

The foregoing facts will give a tolerably correct impression both as to the difficulty of making these observations and of the degree of reliance which can be placed upon them. They are as yet far less exact than one might well wish them to be, but they contain a promise of greater accuracy when more adequate means are employed; and they already enable us to speak positively as to the *direction* of motion of a considerable number of stars, and in the case of not a few to fix the *speed* of motion with some approach to definiteness, some fifty stars in all having been observed a sufficient number of times for us to be able to deduce their speed to the nearest ten miles per second.

An interesting subject for inquiry now presents itself. Do these results afford any support to the theory which holds that the Sun with his attendant planets are travelling towards a point in the constellation Hercules? One or two attempts have already been made to answer this question, but a glance at the annexed diagram (fig. 3) will show at once that these attempts have been premature. A sufficient number of stars have not yet been observed, and those observed are not sufficiently equally distributed to render any inferences as to the direction and speed of the solar motion worthy of much confidence. So far as the observations go, however, they would seem to indicate a motion towards α Aquarii, rather than towards any point in Hercules. Still, if the Sun's speed be small as compared with the average speed of the stars observed, there would be nothing in the observed facts incompatible with the generally accepted direction. Or the suggestion thrown out by Mr. Proctor several years ago may have a foundation in fact, and the stars of Ursa Major may form a connected system having a common drift. Should the stars of Orion's belt, with γ and κ Orionis, form a similar system, the remainder of the stars observed would afford but very incomplete testimony as to the solar motion, but, so far as it went, it would be quite compatible with the usually assumed apex.

The case of the five stars in Orion just referred to is rather interesting, as they seem to be stationary with regard to the Earth, whilst their proper motions also are small. If, then, the spectroscopic results can be sufficiently relied upon, these stars must be

Fig. 3.

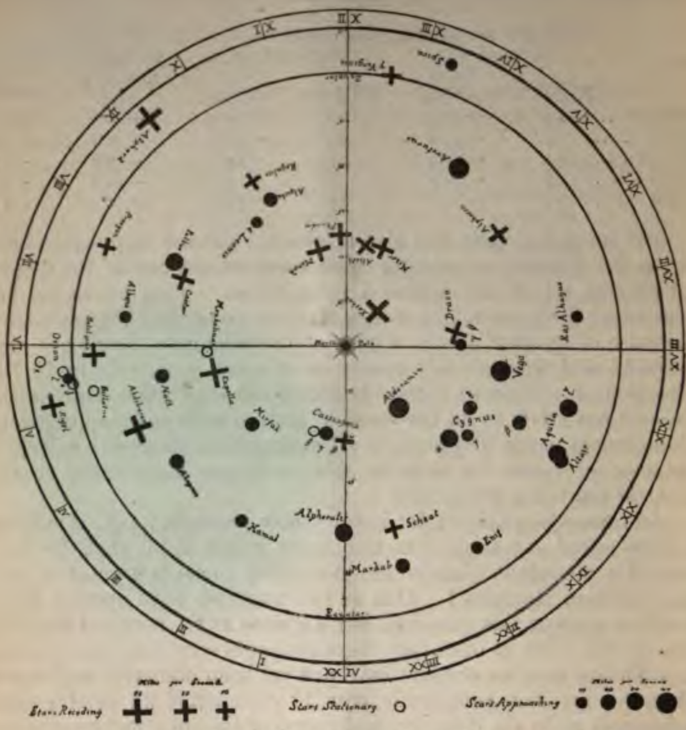


Chart showing distribution of Stars observed for motion in the line of sight.

travelling very nearly in the same direction and with the same speed as our Sun. This would suggest the possibility of their being much nearer to us than is commonly supposed, in which case they might have an appreciable parallax—a parallax, however, that might possibly escape detection if the comparisons were limited to neighbouring stars.

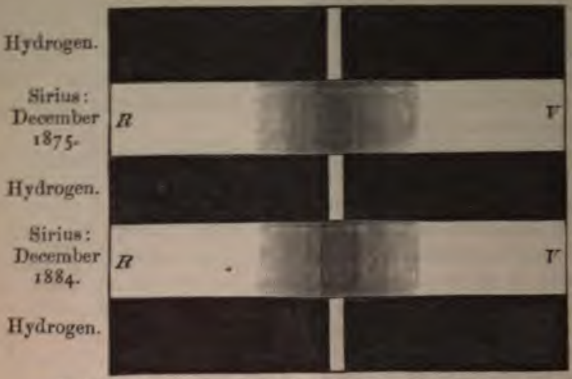
Two other stars than those shown in the little map have been observed at Greenwich a considerable number of times. One of these is Algol, the well-known variable. It will be remembered that Prof. Pickering has suggested that the variations in the light of this star are due to a large dark satellite, which periodically transits across it, producing a partial eclipse. Prof. Pickering has further pointed out that in this case Algol will have a rapid motion round the centre of gravity of the system, and will sometimes, therefore, be approaching us at a high speed, and sometimes receding from us equally rapidly. Up to the present time Algol has been observed on 15 nights. The results show very great discordances; but it is not possible at present to say whether these are due to a real change of motion in the star, or simply to a very unusual roughness in the observations.

The other star is Sirius. In the case of this star the spectroscopic method has brought out a most singular and unexpected result. Each season's observations has given a different value for the rate with which the star is moving, and the change has always been in the same direction. So that whilst in 1868 Dr. Huggins found the star to be receding from us at the rate of 29 miles per second, the observations obtained during the winter just past show it to be now approaching us with a speed of about 22 miles per second. The following table, which was quoted by the President of the Royal Astronomical Society in his address on awarding the Gold Medal of the Society to Dr. Huggins, exhibits the progress of the change very strikingly :—

Opposition of	Number of nights.	Number of measures.	Mean concluded motion.
1875–6 and 1876–77	4	8	+21·1
1877–78	3	8	+23·0
1879–80	3	10	+15·1
1880–81	2	4	+11·3
1881–82	5	22	+ 2·1
1882–83	3	18	− 4·7
1883–84	13	43	−19·4
1884–85	2	8	−21·5

It has already been suggested by Dr. Peters and others, from a consideration of the movements of Sirius in R.A. and N.P.D., at right angles, that is, to the visual ray, that the star is moving in an elliptic orbit, and this would undoubtedly have the effect of making the part of the star's motion which was in the direction of the visual ray vary continually. But it is premature at present to attempt to combine the two different classes of observation into one harmonious whole. Certainly too much weight must not be given to the actual numerical results obtained by taking the mean of the observations of a season. Fig. 4 shows the apparent change in the position of the F line in the spectrum of Sirius, in the nine years between Dec. 1875 and Dec. 1884. It will be seen that it is little more than one-twentieth of the total breadth of the line, so that the mean annual change does not amount to the one-hundredth part of that breadth. And when the diffused and ill-defined character of the line to be measured is borne in mind, and the unsteadiness of the spectrum of Sirius in this latitude, except under the most unusually favourable circumstances, no wonder

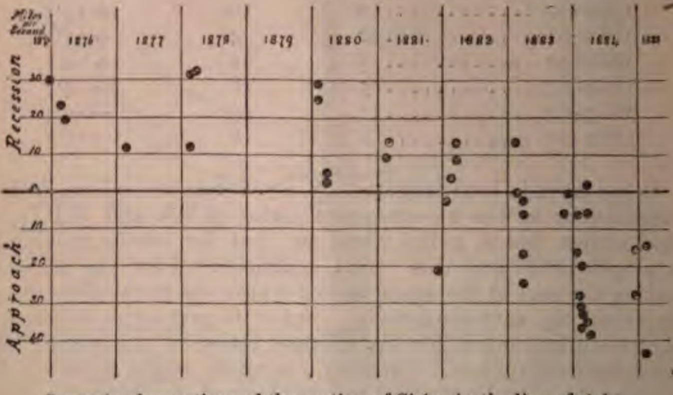
Fig. 4.



F line in Spectrum of Sirius in 1875 and in 1884, as compared with Hβ line of Hydrogen.

can be felt that the mean results do not readily fall into agreement with a given curve. A better idea of the real value of the evidence for the change will probably be afforded by fig. 5, in which each

Fig. 5.



Separate observations of the motion of Sirius in the line of sight, from Dec. 1875 to Feb. 1885.

evening's observation is shown separately. It will be observed that the early observations are but few in number—so few, that if they were not corroborated by Dr. Huggins's observations of 1868 and 1872, it would seem the most natural course to reject them, as possibly affected by some unknown source of error. But since they do possess that powerful confirmation, the fact of the change seems manifest, though the observations are evidently not accurate enough for us to infer the shape of the orbit in which the star is travelling, until the star has much more nearly completed a revolution than it has at present.

Some advance may probably be made in the mean time, either by a resort to larger telescopes, or by the use of some form of spectroscope specially designed for this particular research. The most promising form would seem to be that of a reversion spectroscope, in which half the spectrum is reversed end for end, so that the red of one half is seen immediately above the violet of the other. This has the advantages that the displacement to be measured is doubled in amount, and that the lines in the spectrum of the stars become their own measuring pointers, and all difficulties about the illumination of pointers or cross-wires are done away with. There is also the further advantage that in the case of stars of the first type, the broad diffused line has to be brought into coincidence with a line precisely similar instead of with a wire or pointer entirely unlike it.

The device of dividing the object-glass of the viewing telescope, making it, in fact, a miniature heliometer, would probably give satisfactory results, as the comparison or standard line could be used as a pointer for the bisection of the star-line.

There are several interesting side details which would be sure to attract the attention of any astronomer taking up this line of research. One is the varied character of the same lines in different stellar spectra. Thus Rigel, Cor Caroli, Procyon, and Sirius are all grouped as belonging to the first type of spectrum. Yet there is a well-marked difference in the characters of these lines. Thus the F line in Rigel is narrow and fairly well-defined, in Cor Caroli broad and intensely dark, in Procyon very broad but faint and with a faint narrow line in its centre, in Sirius very broad and very ill-defined, shading off gradually from the centre on either side, but with no trace of anything like a defined line in the centre.

There is a certain degree of similarity in the characters of the F line in the stars of certain districts. Thus the Orion stars except α resemble Rigel, and the principal stars of Ursa Major, except α, resemble the β of that constellation.

There is some slight reason to think that these lines may, in some cases, change their character a little. In two or three cases where the star is variable we know that this really occurs—that, as in the case of β Lyræ as observed by M. Gothard, a line may appear as dazzlingly bright at one time, and may then gradually fade away until it is finally replaced by a dark line not many days later. In the case of γ Cassiopeiæ the change is equally real, though less marked and taking place less quickly. With these instances of change, it seems at least a plausible theory that the character of the lines in other stellar spectra may change too. At all events it certainly is the case that the F line in the spectrum of Altair, which in the years 1874–78 proved exceptionally difficult to bisect, in 1884 seemed little harder than that in the spectrum of Vega, whilst Regulus, on the other hand, has become a somewhat more difficult object for measurement.

The present insufficiency of our means for dealing with the problem of the Sun's path, and the suggestive, but incomplete observations of Sirius, surely justify an appeal to the possessors of great telescopes, and especially to those in southern latitudes to co-operate in a research which promises so rich a reward. Discussions of proper motions and determinations of parallax are needed to increase our knowledge of the structure of the stellar universe; but the thorough application of the spectroscopic method would prove a most valuable additional weapon, and the combination of the three could not fail to secure the most important results. Yet with the one honourable exception of the Temple Observatory, Rugby, where the means are certainly inadequate to the task, the work has been for many years absolutely restricted to Greenwich Observatory, to a single telescope of only barely sufficient aperture, situated where the atmospheric conditions for good observation are rare indeed, and become rarer year by year. If the observatories of, say, Nice and Melbourne and those English amateurs who possess telescopes of apertures of 18 inches and upwards would but lend their aid, without doubt we should soon have results of a considerably greater accuracy than we have at present—results which could not fail to greatly advance our science and increase our knowledge of the sidereal system. E. W. MAUNDER.

Maxima and Minima of Variable Stars observed during the year 1884.

THE observations from which the following results are derived were made with an equatoreally mounted achromatic of 6 inches aperture and powers of 80 and 125.

		Mag.
R Arietis.		
Maximum	Aug. 3	8·1
U Boötis.		
Minimum	April 22	13·3
Maximum	Aug. 1	9·5
V Boötis.		
1855·0 $14^{h}\ 17^{m}\ 18^{s}{\cdot}9\ +16^{\circ}\ 58{\cdot}8$.		
Maximum	June 26	9·4
Minimum	Aug. 7	10·0
R Camelopardi.		
Maximum	May 30	7·9
S Coronæ.		
Maximum	April 30	6·6

S Cygni.

		Mag.
Maximum	Dec. 4	10·1

U Cygni.

Minimum	Sept. 21	11·1

V Cygni.

Maximum	Oct. 19	8·5

X Cygni.

Maximum	Dec. 2	

R Delphini.

Schönfeld's S Delphini.

Minimum	Sept. 18	11·5

S Delphini.

Schönfeld's T Delphini.

Maximum	Sept. 17	8·7

S Herculis.

Maximum	July 24	7·7

T Herculis.

Maximum	May 30	7·4
Minimum	Aug. 23	12·8
Maximum	Nov. 8	6·9

U Herculis.

Maximum	May 31	7·5

R Leonis.

Maximum	Feb. 13	6·6
Maximum	Dec. 20	6·0

R Persei.

Maximum	Mar. 5	7·9
Maximum	Oct. 6	7·9

R Sagittæ.

Maximum, *p.*	May 23	8·7
Minimum, *s.*	June 5	8·8
Maximum, *s.*	June 16	8·7
Minimum, *p.*	July 16	9·4
Maximum, *p.*	July 30	8·7
Maximum, *s.*	Sept. 6	8·8
Minimum, *p.*	Sept. 28	9·4
Maximum, *s.*	Nov. 11	8·85
Minimum, *p.*	Dec. 4	9·95
Maximum, *p.*	Dec. 23	8·5

The letters *p* and *s* indicate principal and secondary maxima and minima.

[illegible]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Observation of the Red Spot on Jupiter.

Gentlemen,—

I have seen no notice of a remarkable peculiarity which h
recently developed itself upon the surface of the red spot.
Its central portion is now covered by a white cloud of oval for
which is almost concentric with it, and leaves visible only a narro

ring of the red substance, not exceeding 1½″ to 2″ in width. The outline of the cloud is for the most part sensibly smooth and regular; but at the preceding end there is a little projection, which almost, and possibly quite, cuts across the red ring. If, however, there is really a connection here between the interior of the red spot and the region outside, the channel must be very narrow. When the seeing is best, and with the highest powers (950 and 1200), the red ring appears to be as yet unbroken.

The white cloud is not brilliant, but is very nearly of the same tint as the white belt on which the red spot lies, so that if the cloud ever comes to cover the spot entirely, it will completely obliterate all trace of its position, except the notch in the southern equatoreal belt which now marks the eastern end.

I first saw the white cloud a week ago (March 23rd), but it may have been visible much longer, as I had been away from home for several weeks, and in fact had not happened to look at the spot-side of Jupiter since the opposition. With the 23-inch equatoreal the cloud is unmistakable, and easily enough seen under fair atmospheric conditions; I have observed it on the 23rd, 25th, and 26th, and on the 26th I could make it out also with the 9½-inch instrument of the students' observatory, but it was a delicate and difficult object.

May I add a word (apropos of Mr. Denning's paper in your March number) as regards the visibility of planetary markings in large and small telescopes? Much that Mr. Denning says as to the greater susceptibility of large instruments to atmospheric disturbances is most sadly true; and yet on the whole I find also true what Mr. Clark told me would be the case on first mounting our 23-inch instrument, *that I can almost always see with the 23-inch everything I can see with the 9½-inch under the same atmospheric conditions, and see it better*:—if the seeing is bad, only a little better; if good, immensely better. The only exceptions are in the case of objects which require a very low power, lower than any that can be obtained with the eyepieces of the large telescope.

I find this also, that frequently markings on Jupiter and Saturn, which with the smaller telescope appear most beautifully definite and sharp, turn out when examined by the larger one to be quite different, hazy in outline, and made up of an assemblage of finer details. This is due to no fault of either instrument; I do not believe there exists any more perfect object-glass of its size than our 9½-inch Gaussian lens; but at the best, from optical necessity, it must represent, in the image it forms, a mathematical line by a stripe about 0″·4 wide, and a point by a disk of the same diameter, while in the image formed by the large glass the corresponding figure is less than half as much, about 0″·18; and so a shaded patch, as seen in the smaller telescope, often turns out to be a cluster of stipplings, or a range of lines. Under high powers also markings which are conspicuous with lower ones often disappear, in the same way that the naked-eye markings on the Moon vanish in the telescope.

Most commonly, however, when I have failed to see with th large instrument anything I supposed I saw with the smaller, has turned out on examination that the larger instrument w right, and that imagination had constructed a story that was n true by building up faintly visible details and hazy suggestio furnished by the smaller lens.

Princetown, N.J., U.S.A. C. A. Young.
March 30, 1885.

The Defining Power of Telescopes.

Gentlemen,—

I hope the interesting remarks of Mr. Denning in th Journal, March 1885, may direct the attention of astronomers t the question of the relative powers of large and small telescope in showing faint objects on the surfaces of the planets. There i perhaps, too much scepticism on the part of those who are observin with large instruments in regard to what can be seen with smal ones. During the past eleven years I have observed Saturn wit the Washington 26-inch refractor at every opposition, and hav made notes of the appearance of the planet, of the ring, and o the shadows. It is plain that my work will confirm Mr. Denning's criticism that the large telescope does not show enough detail. With the exception of the white spot that appeared near the equator of the planet in December 1876, I have seen no remarkable change on its surface during the past eleven years. This constancy may be reckoned singular when we consider the small density of Saturn. Another phenomenon which has been carefully looked for, and which I have never seen, is the notch in the shadow of the ball on the ring. This notch is now always given in every good book on popular astronomy, and it has been seen by several observers since my work began; but notwithstanding the most careful attention, I have never seen it. Also the square-shouldered appearance of this planet, which visitors frequently ask for, has never been seen here.

There is another point in connection with this question that should be considered. It is the discovery of small companions of Polaris, and of stars inside the Trapezium of Orion, so frequently made with telescopes of five or six inches aperture.

Washington, April 4, 1885. A. Hall.

Satellite IV. of Jupiter.

Gentlemen,—

At the March meeting of the Society I read a brief note of the appearance of the IV. Sat. of Jupiter during a black transit, to the effect that it appeared quite round at 9.45, but oval at 10.20. The observation was not intended to have been thought unique, for Lassell saw the same thing on the 5th March, 1850, only in this case the long axis was N. and S., whereas it appeared to me parallel to the belts. It was suggested that perhaps my instrument (10-inch reflector) was out of adjustment; but I hardly think this could have taken place in an hour.

The question as to whether I was astigmatical was then raised, and it is upon this point I am desirous of pointing out that it would have made no difference had I been so affected. The prevailing idea that this disease causes those affected to see a round object elongated is, I believe, incorrect, as can be easily proved by a simple experiment first suggested by Mr. Brudenell Carter, the well-known occulist. A cylindrical lens is placed before the eye—say with the long axis vertical—in such a position as to *almost touch* the cornea; a round black object now viewed does *not* appear oval, but simply black at the upper and lower portions, dusky and ill-defined towards the right and left; and, moreover, the elongated effect is not produced until the lens is shifted some distance from the eye, when it becomes very marked. In his work upon Diseases of the Eye, Mr. Carter explains this experiment fully; briefly it is this:—In an eye myopic say for vertical lines, the major axis of the supposed ellipse, and the greater length of the apparent oblong, would be in a horizontal direction; but for this to be really appreciable, a considerable distance must intervene between the disturbing or astigmatical medium and the screen on which the image is received; just as, for instance, the distortion of a shadow is increased by the distance of the surface on which the shadow is cast. Between the cornea and the retina the distance is not sufficient (except perhaps under some most extraordinary circumstances) to produce any lateral elongation such as would be recognizable by the senses, hence astigmatics do not see objects elongated, but merely ill-defined at some parts and well-defined at others. The idea has become prevalent owing to experimentalists having placed the lens too far from the eye.

Two Fellows present thought the black satellite appeared round; this apparent discrepancy is not uncommon, perhaps arising from the attention of one or other observer being not specially directed to the question of shape; for in March last year, during another black transit, the satellite appeared round to Mr. Knobel and myself, but irregular to Capt. Noble, and elongated to Mr. Pratt. I attempted to verify the appearance in the reflector by using a small Tulley refractor, but clouds prevented any further observation. I think, however, the phenomenon was not instrumental, seeing the satellite appeared round an hour before.

Ivy House, Clapham Common. EDMUND J. SPITTA.

On the supposed Proper Motion of the Nebula h 705.

GENTLEMEN,—

In your last number Mr. Sadler has drawn attention to the nebula *h* 705=G. C. 2091, contending that "irregular but certain motion seems well established." When the observations of this object are examined with care, it appears, however, that there is no certain evidence of proper motion.

[illegible] as affecting [illegible] at the Janua[illegible] Tr[illegible] Systemat[illegible] of the Cap[illegible] intended b[illegible] value of Mr. Stone[illegible]

[illegible] that such an attempt was ver[illegible] sensible than I am [illegible] that Catalogue. The large [illegible] as follows:—"Great [illegible] and I have no doubt it [illegible] by Mr. Gill."

ber of stars which it contains, the stability and optical power he instrument employed, the tried experience and skill of the ervers, the systematic arrangement and minute accuracy of the merical computations, together with the care with which it has en passed through the press, combine, in my opinion, to render the most important contribution to Sidereal Astronomy that has manated from the hands of an Englishman since the days of Bradley.

Because the Catalogue is already so valuable it is important that astronomers should be fully informed on all points which affect its utility, *i. e.* all points which affect its errors (accidental or systematic), and the range within which such errors are confined.

It is only with such knowledge that the results of any series of observations can be legitimately employed, with their due weight, in future researches; and the want of such knowledge must always tend to impede progress in our search for truth.

Having discovered that the new screws (made by Mr. Simms for the Cape Circle in 1880) had acquired serious errors produced by wear in the course of less than five years, it naturally became important to examine whether larger errors might not have been caused by wear in similar screws which had been in use from 1855 to 1879. The results of examination of the new screws, and such evidence as could be obtained about the errors of the old screws from the records of the Observatory, I therefore collected for publication; and as Mr. Stone had published what appears to me erroneous views about the origin of these errors, I went into considerable detail on the subject.

These, and these only, were the considerations which induced me to write the papers in question.

To any man honestly in search of truth, the indication of an error in his work is fully as valuable as an additional proof of its accuracy. It seems to me that there is nothing more valuable to an observer than fair criticism, and nothing more instructive to the critic than to have an error in his criticism pointed out.

Such discussions generally result in good. It was by similar discussion and criticism by Mr. Christie, in the matter of Bessel's Refractions, that Mr. Stone was led to make the series of observations which now afford the most valuable data for discussion of the error of the Circle Screws in 1877. There is, indeed, a tendency in work which involves much routine to degenerate into mere routine, and there is no better corrective to such an unfortunate tendency than fair criticism and questioning from time to time.

But if fair criticism is to be met, from the Chair of the Royal Astronomical Society, by such remarks as those I have quoted, then I think we are likely to drift into a state of affairs not conducive to the progress of Astronomy. DAVID GILL.

Royal Observatory, Cape of Good Hope,
1885, March 16.

[We have printed Dr. Gill's remarks in full, but at the same time we are fully assured that he is wholly mistaken in fancyin that the President regarded his paper "simply as an attempt t lower the value of Mr. Stone's Catalogue." We speak the mor confidently on the subject, because Mr. Dunkin himself requeste us, unfortunately not until the number was passing through th press, to alter the sentence complained of, on the very ground tha it might *possibly* be misunderstood in the sense which Dr. Gill ha placed upon it. We are assured that Mr. Dunkin wished simpl to express his dissent from some of Dr. Gill's conclusions, and tha he had not the remotest wish, directly or indirectly, to cast an reflection upon his motives.—EDITORS.]

Note on the Solar Eclipse, 1885, *March* 16.

GENTLEMEN,—

The first contact was observed at Washington, D.C., and the times given by three observers were as follows:—

$$\text{March } 15^{d}\ \left.\begin{array}{lll} 23^{h} & 57^{m} & 6^{s}{\cdot}9 \\ 23 & 57 & 7{\cdot}5 \\ 23 & 57 & 4{\cdot}6 \end{array}\right\}\text{Mean local time.}$$

The time subsequently determined from photographs was $23^{h}\ 57^{m}\ 6^{s}{\cdot}2$.

The mean of all these is $23^{h}\ 57^{m}\ 6^{s}{\cdot}3$.

The time of first contact computed by the writer from the coordinates of the Sun and Moon, as given in the American Ephemeris and Nautical Almanac, was

$$23^{h}\ 57^{m}\ 7^{s}{\cdot}5,$$

which differs from the mean, as above determined, by $1^{s}{\cdot}2$. So clos an agreement between observation and computation is not only remarkable, but also attests the accuracy of the positions of the Sun and Moon as given in the American Ephemeris.

The last contact took place at March 16, $2^{h}\ 38^{m}\ 14^{s}{\cdot}1$, but wa not observed here, owing to the sky being overcast with clouds.

Washington, D.C., J. MORRISON.
1885, March 27.

U Cephei.

GENTLEMEN,—

In the article on the "Photometric Observations of Nep tune at the Harvard College Observatory," which appears in th April number of the 'Observatory,' Prof. Pickering has som important remarks on "the influence of relative position upon th observed brightness of the objects compared," and applies them t the case of U Cephei, as perhaps having a bearing on the difference observed by myself in the character of alternate minima.

The caution is an important one, and it would probably be pre mature to express a too-decided opinion on the subject at present At the same time it should be mentioned that my observed magni tudes do not depend solely on the estimated difference between th variable and selected comparison stars, but are checked by directly gauging the star's magnitude by the method of limiting apertures, a method which would be unaffected by the disturbing cause referred to.

I may add that conclusions similar to my own in regard to the light-curve of this variable appear to have been arrived at by Mr. Chandler, and by Dr. Wilsing of the Potsdam Observatory. An interesting paper by Dr. Wilsing, entitled "Untersuchungen über den Lichtwechsel von U Cephei," will be found in No. 2596 of the 'Astronomische Nachrichten.' I am, Gentlemen,

Yours faithfully,
GEORGE KNOTT.

Knowles Lodge, Cuckfield,
1885, April 18.

Abnormal Appearances of the Satellites of Jupiter.

GENTLEMEN,—

I shall be much indebted if you will kindly allow me to request any of your readers who possess notes of abnormal appearances of any of Jupiter's satellites, during transit especially, to forward the same to my address, as I am collecting unpublished observations. I would ask particularly whether any observer has ever seen Sat. IV. during transit otherwise than black, chocolate, or steel-grey in colour, or Sat. II., under similar conditions, otherwise than white. If so, in giving me the note, will they kindly mention the exact date and time, if possible the temperature at the time of observation, and whether the instrument was a reflector or refractor, its aperture, and the power employed?

Yours faithfully,
EDMUND J. SPITTA.

Ivy House, Clapham Common,
1885, April 17.

NOTES.

PROPOSED ALTERATIONS IN THE BYE-LAWS OF THE ROYAL ASTRONOMICAL SOCIETY.—We would draw the attention of all Fellows of the Society to the circular recently issued by the Council calling a Special General Meeting to be held on Friday, May 8, for the purpose of considering the following proposed alterations in the Bye-laws:—

"1. That at the end of Bye-law 12 the following words be added:—

'Lists handed to the Scrutineers of the Ballot on behalf of any Fellow not present at the Meeting shall be accepted if duly verified by the signature of the absent Fellow.'

"2. That at the end of Bye-law 53 the following words be added:—

'Fellows of the Society may vote on the business brought before Annual General Meetings and Special General Meetings either personally or by proxy.'"

[illegible] alteration in the hour of th[illegible] [illegible] taken into consideration. It is [illegible] every Fellow who can possib[illegible] [illegible] important that the Meetin[illegible] [illegible] should be a thoroughly and [illegible]

New Variable Stars.* — [illegible], whilst searching for minor [illegible] by the aid of Chacornac's [illegible] variables. These stars [illegible] established with [illegible]. The positions are referred [illegible]

N°.	R.A. h	m	s	Dec. °	'	Obs. Mag. max.	min.	N°.	R.A. h	m	s	Dec. °	'	Obs. Mag. max.	min.
1	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	[illegible]	[illegible]	14	21	[illegible]	[illegible]	− [illegible]	[illegible]	[illegible]	9
2	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	[illegible]	[illegible]	15	21	[illegible]	[illegible]	− [illegible]	[illegible]	12	<13
3	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	[illegible]	[illegible]	16*.	21	[illegible]	[illegible]	− [illegible]	[illegible]	12	<13
4*	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	[illegible]	[illegible]	17	21	[illegible]	[illegible]	− [illegible]	[illegible]	12.5	13
5	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	[illegible]	[illegible]	18.	21	[illegible]	[illegible]	−14	[illegible]	11	<13
6.	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	[illegible]	[illegible]	19*.	21	[illegible]	[illegible]	− [illegible]	[illegible]	11	12
7	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	[illegible]	<13	20*	21	[illegible]	[illegible]	− [illegible]	19	12	12
8*.	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	[illegible]	[illegible]	21*	21	21	[illegible]	− [illegible]	[illegible]	11	13
9	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	12	<13	22	21	21	[illegible]	−12	44	11	<13
10	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	11	[illegible]	23*.	21	22	[illegible]	− [illegible]	[illegible]	11	<13
11	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	11	<13	24*.	21	22	[illegible]	−12	44	11	<13
12.	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	[illegible]	[illegible]	25.	21	22	[illegible]	− [illegible]	[illegible]	11	<13
13*.	[illegible]	[illegible]	[illegible]	− [illegible]	[illegible]	12	<13	26*.	21	22	[illegible]	− [illegible]	59	11	<13

It is [illegible] by Herr Kreutz, in some remarks on the above list, which he has [illegible] 'Bulletin Astronomique,' Tome ii [illegible] that N[illegible] R Leonis, the variability of which wa[illegible] discovered by K[illegible]

The Temple Observatory.—From the Report of this Obse[illegible] [illegible] that Mr. Seabroke is continuin[illegible] his excellent work at Rugby. During the year the original wo[illegible] [illegible] consisted in the measurement [illegible] stars. 264 complete measures [illegible] stars having been taken. Mr. Seabroke has prepared a ne[illegible] list of stars for [illegible] years. Some few measu[illegible] [illegible] motion of stars in the line [illegible] reflector. The observations [illegible] presented to the Ro[illegible] [illegible] printed in the Memoi[illegible] [illegible] results of the motions of stars in the li[illegible] [illegible] as we believe that these are t[illegible] [illegible] made except those at Greenwi[illegible] [illegible] Mr. Seabroke will publish his results, a[illegible] [illegible] comparison.

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* [illegible] Astronomique,' Tome ii. pp. 61–63.

THE SUN AND HIS PHENOMENA *.—This neat little book, by the Rev. T. W. Webb, is intended to be used in elementary instruction. Its style is somewhat too obviously tutorial, and is rather that which would have been employed in bygone days than that which finds favour for juvenile instruction now. But the book gives a clear and accurate account of the principal facts known about the Sun, and of the methods in which those facts have been ascertained. We do not know whether the popular taste is at present disposed to prefer little manuals, dealing with particular departments of a subject, to larger works embracing the whole; we fear not. But if that be the case, we trust Mr. Webb's little book will meet with the success which the care bestowed upon it deserves; and that its author may be encouraged to complete his task by other similar works dealing with the remaining members of our system.

A CORRECTION.—Maj.-Gen. Tennant writes to us to correct his statement that Mr. Common's five-foot glass disk cost him £300 ('Observatory,' No. 96, p. 102). The actual price was £130. Gen. Tennant also had not intended to say that Greenwich mean time had its origin at midnight (p. 106). He had wished to prevent the confusion arising from calling the Civil English day, which is to be the Universal day, by a name which has a definite meaning attached to it.

VARIABILITY OF SATURN'S RINGS.—We have received a very complete and valuable paper by M. Trouvelot on the variability of Saturn's Rings. The paper, which appeared in the 'Bulletin Astronomique' for Nov. 1884 and Jan. 1885, is an expansion of a note communicated to the Académie des Sciences in May 1884, and which was noticed in the 'Observatory,' No. 86, p. 178. The principal points brought out by M. Trouvelot in the present paper may be briefly summarized as follows:—

> Ring A usually shows one dark line on either ansa (Encke's division). This line is not stable. It has recently disappeared whilst another line, more distinct and nearer to the Cassinian division, has been seen instead. On some occasions several fine lines have been seen, and not merely one alone. And when Encke's division has been seen alone, it has not always occupied the same position on the ring, nor even been always symmetrically placed on the two ansæ. The ring has also shown variations of brightness, one ansa sometimes appearing brighter than the other. Recently, too, a white band has appeared upon the ring, so conspicuous that the brothers Henry expressed their belief that Encke's division had no real existence, but was a purely subjective phenomenon caused by the contrast between this bright band and the general surface of the ring. The shadow of ring A has also

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* The Sun: a familiar description of his phenomena. By the Rev. Thomas William Webb, M.A., F.R.A.S. London: Longmans, Green, & Co. 1885.

shown change. Usually black as ink, it has been seen of a greyi
or dusky tint, or sometimes partly grey and partly black. Fr
this and from other circumstances M. Trouvelot infers that ri
A is occasionally partly transparent.

The Cassinian division has shown changes of breadth and shap
the outer edge is sometimes sharply defined, sometimes shaded a
diffused. At other times it shows dentelures which invade t
outer ring A. M. Trouvelot also cites observations which sh
the Cassinian division not to be entirely black, and therefo
probably not entirely free from matter.

Ring B usually shows three zones of about equal breadth, t
interior being the least, and the exterior the most brilliant. The
zones vary from time to time in breadth and brilliancy, and a
frequently of unequal brightness and distinctness on the two ans
Several narrow dark rings have also been detected on rar
occasions.

The shadow of the planet on the ring has shown some most in
teresting variations, its outline being sometimes straight, sometime
curved, sometimes irregular. M. Trouvelot describes these variou
shapes at considerable detail and remarks that they can only b
explained by changes of level in the ring, and he infers from th
usual form of the shadow that both the upper and lower surface
of ring B are convex. Similar changes have been noticed in th
shadow on ring A, but these are much more difficult to observe.

The crape ring C, though less easy to observe than the tw
brighter rings, undergoes as they do sudden and constant varia
tions. Sometimes one ansa is readily seen, whilst the other i
scarcely visible. The ring is sometimes so dark that the shado
of the planet cannot be detected upon it, at other times the shado
is readily seen. The outline of the shadow varies also—sometime
it is straight, sometimes concave, sometimes even convex. Th
inner edge of the ring will at one time appear sharp and wel
defined, at another diffused and shaded. The transparency of thi
ring has often been remarked, but according to M. Trouvelot it i
now transparent through not more than half its breadth, thoug
there is strong evidence to show that it was transparent right u
to ring B some thirty years back. M. Trouvelot also discusses a
considerable length the cause of the apparent shape of that par
of the crape ring seen on the planet, and concludes the paper b
pointing out that the hypothesis that the rings are composed o
swarms of minute meteorites of different reflecting powers satisfie
all the observed phenomena.

Observers of Saturn owe much to M. Trouvelot for thus colla
ting so large a number of observations of this beautiful planet, an
bringing out so clearly the principal points in the ring system t
which attention should be directed.

THE ASTRONOMICAL PRIZES OF THE ACADEMY OF SCIENCES*.—
The Lalande Prize has been awarded to M. Radau for his variou

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* Comptes Rendus, Tome c. No. 8.

astronomical works, more particularly for his memoir on the theory of astronomical refractions. The Valz Prize has been decreed to M. Ginzel for his discussion of 43 ancient eclipses of the Sun, total or annular, concerning which he has been able to collect fresh documents. M. Ginzel has derived from this discussion a new determination of the numerical value of the secular acceleration of the mean motion of the Moon, which confirms the difference between observation and theory, but which slightly lessens its amount.

Carbon Bisulphide Prisms.—The April number of the 'American Journal of Science' contains an interesting account of some experiments made by the late Dr. Henry Draper, whose death was so severe a loss to science, on prisms filled with carbon bisulphide. Dr. Draper had used some prisms lent him by Mr. Rutherfurd for his researches as to the presence of oxygen in the Sun. As used in this investigation, the behaviour of the prisms was all that could be wished, the temperature of the room in which the photographs were taken being remarkably uniform. But when the prisms were transferred to Dr. Draper's new laboratory, no definition whatever could be obtained with them, owing to the rapid variations of temperature there, and their use had to be abandoned. But finding that both gratings and spectroscopes with flint prisms were open to objections from which the bisulphide prisms had been free, Dr. Draper tried to devise some method of overcoming the bad definition caused by the changes of temperature; and he finally found that, by an active agitation of the liquid, he could destroy the striæ caused by convection-currents and obtain exceedingly fine definition. He therefore fitted a small propeller-wheel in the bisulphide, driven by a small electric motor. In this way the definition obtained was so good that even with a faint flame the sodium line could be seen distinctly reversed. But, unfortunately, the temperature-changes introduced another difficulty, the dispersive power of the prisms suffering great alteration, so that a fall of 9° F. caused a shift towards the violet of 0·75 inch in the sodium lines. Dr. Draper, however, devised an equal-temperature box which overcame this drawback, a fatal one in photographic work. The box was heated by a gas-flame, the temperature within it being regulated by two compound expansion bars included in the circuit of a battery and an electromagnet so arranged that when the ends of the bars came in contact as the temperature rose, the circuit was closed and the electromagnet turned down the gas. The apparatus was further improved later, so that the temperature did not vary 0°·1 in seven hours. Dr. Draper thus finally overcame the hindrances to the successful use of bisulphide prisms, though, to the great loss of our science, be did not survive to use the perfected apparatus he had devised.

Prof. Young on the Solar Corona.—Two of our highest authorities have recently expounded their views as to the nature of the Solar Corona—Dr. Huggins, in his lecture before the R Institution, and Prof. Young, in a recent article in the 'N American Review.' It is gratifying to find that these two g astronomers are agreed upon the most important points. Prof. Young, on the chief question, states "his complete viction that the corona is mainly solar." He points out tha corona sends us three kinds of light, the spectroscopic evic showing that reflected or diffused sunlight is present, though the main constituent of the coronal luminosity, the contin spectrum indicating the presence of incandescent particles, sol liquid, whilst the bright lines show the presence of luminous g These mingled gases form what has been called the "coronal a sphere," but in it "there are filaments and streamers and forms that are probably not gaseous, but composed of mist dust: some of them may be of meteoric origin, and some comp of matter ejected from the Sun, while others perhaps are du the condensation of vapours." The analogy of the channels in tails of comets to the coronal rifts, and the evidence comets a to a repulsive force exercised by the Sun, strike Prof. Youn they do Dr. Huggins, and the suggestion which the latter w out so well as to the probable extreme rarity of coronal matt also thrown out by the former. "All the luminous phenomen the corona could be accounted for by an atmosphere of a de millions of times below that in any vacuum-tube ever construc Prof. Young, however, gives Prof. Hastings' theory a more fa able reception than does Dr. Huggins—more favourable, we ver to think, than it deserves; but adds that, "Of course, if the co photographs of Dr. Huggins are accepted, and if they should after be confirmed by new ones of greater clearness, that w close the discussion." This additional evidence Prof. Young siders is furnished by Mr. Ray Woods' report as to the resu his Riffelberg experiments given in his article in No. 92 o 'Observatory' (Dec. 1884), and his final conclusion is thus in stantial accord with that of the English astronomer.

FAYE'S COSMOGONIC THEORY.—Prof. G. H. Darwin has co buted to 'Nature' (Vol. xxxi. p. 506) an interesting and val criticism of M. Faye's theory of the evolution of the solar sy as explained in his paper in the 'Annuaire du Bureau des L tudes' for 1885, and also in his work entitled 'Sur l'Origi Monde.' The best general idea of the line of speculation ad by M. Faye may be given by saying that it is a theory of evol from meteorites, instead of from the nebulous matter which its name to Laplace's theory. In the primitive condition universe consisted of matter scattered in chaotic confu Currents were generated in the midst of this chaos under th fluence of mutual gravitation; and in consequence of these n ments, shreds of matter became detached and moved with linear and slow gyratory motion. The solar system is take have originated from a shred which aggregated into a spheroidal shape and consisted, at the epoch when we begin to watch it, largely of separate meteorites. It is at first supposed that the spheroidal aggregate consists of matter pretty nearly equally distributed, and later a nucleus is formed. If r be the distance of any point from the centre, the force is central, and follows the law $ar+\frac{b}{r^2}$, where, in the beginning of the evolutionary process, b is very small, and later a becomes small. Initially, then, when the force is simply as the distance, each meteorite moves in an ellipse about the centre, and the periodic time of all is the same, whatever their eccentricity of orbit. In consequence of collisions, a central nucleus is soon formed; as this increases, the a in our formula for the force diminishes and the b increases, but orbits which are circular still retain that form, notwithstanding the progressive change in the law of force. At the same time that the nucleus is being formed, a series of flat and nearly circular rings arise around it, those near to the nucleus attaining a definite shape sooner than the remote ones. It is not adequately explained why the matter should be sifted, and should arrange itself in rings at definite intervals around the nucleus, still less is any light thrown on the law of Titius concerning the distances of the planets from the Sun. Considering now the case of the first ring, M. Faye supposes that slight differences of angular velocity, mutual attraction, and collisions gradually cause the aggregation of all the matter in the ring around some centre in its line. When the nucleus is small, the rings moved as a rigid whole, and the linear velocity of the outer meteorites was greater than that of the inner ones; therefore when the planetary aggregate is formed, it will be found rotating with direct motion about an axis nearly perpendicular to the plane of its orbit. As we proceed from the first ring outwards, in each successive case the tendency to direct motion is weaker, because the increase of the solar nucleus by absorption of meteorites has prevented so large an excess of linear velocity of the outer meteorites over those of the inner ones as in the first case. By degrees, therefore, we come to planets in which the meteorites move nearly according to Kepler's laws, and here the resulting planet has a retrograde rotation. Each planetary agglomeration in its turn forms a miniature solar system, and generates satellites by the same process as that in which the planets were formed. After having thus sketched M. Faye's theory in its main outlines, Prof. Darwin points out that no reference is made to the possible effect of tides in the evolution of the solar system, a part of the subject which has been so ably worked out by Prof. Darwin himself. He has shown that the hypothesis that tidal friction has had free play in the past leads to a remarkable quantitative co-ordination of the several elements of the Earth's rotation and of the Moon's orbital motion, and points to the genesis of the Moon close to the present surface of the Earth. The rapid orbital motion of the inner satellite of Mars confirms in a remarkable way the truth of the hypothesis. It also throws light on the cause of the observed distribution of satellites in the solar system. It is, in Prof. Darwin's opinion, a factor which cannot be left out of account, and has a bearing on theories of evolution which cannot be neglected. Prof. Darwin's paper concludes with a summary of the advantages and disadvantages of M. Faye's scheme. The conception of the growth of planetary bodies by the aggregation of meteorites is a good one, and perhaps more probable than the hypothesis that the whole solar system was gaseous. The internal annulation of the meteorites is left unexplained, and this compares very unfavourably with Laplace's system, where the annulation is the very thing explained. The difference of orbital motion of the inner and outer meteorites of a ring, the development of that difference as time progresses, and the consequence of direct and retrograde rotation at different distances from the Sun, is an excellent idea. But it is necessary to this idea that the inner planets should have been formed first *, and we are met directly by the fact that the single surviving ring, that of Saturn, is nearer to the planet than are the satellites, and we should be driven to the startling conclusion that Saturn's ring is the oldest feature of his system. The actual distribution of satellites in the solar system is at variance with M. Faye's theory, for as, according to him, the internal planets were generated from rings whose motion was such as would give greater moment of momentum to the planetary agglomeration than would the external ones, the number of satellites should be greater the greater the amount of rotation in the primitive agglomeration of meteorites, and thus the nearer planets should be richer in satellites than the remote ones. On the whole, then, there are great difficulties in the acceptance of M. Faye's scheme, notwithstanding its excellencies; but science is undoubtedly the gainer by such suggestive theories.

THE ADJUSTMENT OF OBSERVATIONS †.—In this treatise the author gives a systematic account of the method of adjusting observations founded on the principle of the mean. Starting as Gauss did from the arithmetic mean as "a plausible hypothesis" he deduces the Gaussian Law of Error, and proceeds to explain the deduction and use of "error of mean square" and of "probable error" in the usual way. He explains clearly and well the application of these to (1) direct observations of one unknown; (2) indirect observations of several independent unknowns; (3) conditioned observations. The method of least squares is also fully treated of, and its application excellently illustrated (as are also the other processes explained in the treatise) by many numerical examples.

* In fact, according to M. Faye, the Earth is older than the Sun. "If it were otherwise, the whole appearance of the sky would be changed; the stars would rise in the west, and set in the east; the Moon would have a retrograde motion like the Satellites of Uranus and Neptune" (Sur l'Origine du Monde, p. 192).

† A Treatise on the Adjustment of Observations. By T. W. Wright, B.A., Civil Engineer. Van Nostrand: New York, 1884.

With reference to the various so-called proofs of the law of error, special stress is laid on the *à posteriori* proofs—that is, the agreement of the observed frequency of error in an actual series of observations with the theoretical law. Such an agreement has been exhibited by Sir G. Airy, in the appendix to the third edition of his 'Theory of Errors of Observations,' in the case of 636 observations of N.P.D. of Polaris made at Greenwich during the years 1869–1873, and has been shown to exist in other series of observations also, to which Mr. Wright refers. This will doubtless be considered the most satisfactory method of proof, at all events by the practical observer. Mr. Wright informs us that "the first demonstration of the exponential law of error was published in 1808 by Dr. R. Adrain of Reading, Pa., in the 'Analyst' or 'Mathematical Museum.' He gives two demonstrations, the second being essentially the same as that now known as Herschel's proof." This appears to be the same as that given in Thomson and Tait's 'Natural Philosophy,' first edition, p. 314, and by Airy on p. 7 of the work referred to above. It is perhaps the most satisfactory of any of the *à priori* proofs of the law. Gauss's demonstration was not published until 1809, so that he was clearly anticipated by Mr. Wright's fellow countryman. Though primarily intended for geodesists the book (at all events the first four chapters of it) may be studied with advantage by astronomers seeking information on the various points connected with the adjustment of observations, who will find many hints and suggestions scattered throughout the treatise of great value to the practical computer. Useful tables of the factors for Bessel's and Peters' probable-error formulæ are given at the end of the volume. To these might advantageously have been added a table of squares, similar to those given by C. F. W. Peters or by Beardmore.

THE REDUCTION OF OBSERVATIONS OF CLOSE POLAR STARS [*].—M. Loewy has communicated to the 'Académie des Sciences' some remarkable and interesting results which he has arrived at with regard to the observations of transits of circumpolar stars. The ordinary methods of reduction take account of the instrumental errors in a very imperfect way; so that whether the object is to determine the time of passage of the star over the meridian or to determine the collimation error from observation of a circumpolar, the results are liable to considerable inaccuracy if the star be in the immediate neighbourhood of the pole, or be observed at a considerable distance from the meridian. M. Leowy shows how the "corrective term" to the observed time of the star's passage is deduced, and gives a table of its value for each minute of hour-angle from $0^h\ 0^m$ to $1^h\ 20^m$, and for each minute, 1^m to 10^m, for the value of the argument $n \tan \delta + (c-k) \sec \delta$, where k is the diurnal aberration, and n and c the usual instrumental constants. It is noteworthy that the most important part of the correction arises from the circumstance that in the ordinary reduction we put cos $\tau = 1$, where τ is the star's hour-angle with regard to the plane of the instrument. Taking for $n+c$ the value of 2^s, and for the *true* hour-angle the value 20^m, this introduces an error of reduction equal to $0^s \cdot 44$ for the star B.A.C. 2320 (N.P.D. for 1850=0° 58′); and equal to $1^s \cdot 4$ and $5^s \cdot 5$ respectively for stars situated 20′ from the pole and 5′ from the pole. If hour-angle $=30^m$, the errors at these three polar distances become respectively $1^s \cdot 3$, $3^s \cdot 5$, and $13^s \cdot 8$. M. Loewy remarks that hitherto the study of stars very close to the pole has been little attended to on account of the difficulties of observation and of reduction, and on account of the uncertainty which exists on the subject of the employment of approximate formulæ. These researches are well calculated to remove such obstacles, and to show that observations can be made without additional complication in the reductions, at considerable distances from the meridian. In fact in some cases (such as the determination of the instrumental constant n) it is not only advantageous, but even necessary for the attainment of great accuracy, as M. Loewy shows, that the transits of circumpolars should be observed at a great distance from the meridian. We trust that an impetus will be given by M. Loewy's researches to the prosecution of the important work of the observation and reduction of transits of stars in the immediate neighbourhood of the pole.

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[*] Comptes Rendus, Tome c. Nos. 7 and 10.

STAR WITH LARGE PROPER MOTION*.—Dr. Gould announces that the 8th mag. star number 1584, in hour xxiii. of the Cordoba Zone-Catalogue, proves to have a proper motion to an extent only inferior, among those known, to that of Groombridge 1830 and of Lacaille 9352. From the Cordoba observations, extending from 1872·9 to 1884·8, Dr. Gould infers the annual motion of $+0^s \cdot 4823$ in R.A., and $2'' \cdot 4479$ in decl. This corresponds to $6'' \cdot 2057$ in arc of a great circle, in a direction 66° 46′ to the east of south. The observations give for the epoch 1875·0 :—$\alpha = 23^h\ 58^m\ 1^s \cdot 85$, $\delta = -37^\circ\ 58'\ 18'' \cdot 76$.

CHANGE IN THE ASTRONOMICAL DATE.—From the replies to a circular, issued from the Naval Observatory, Washington, we gather the opinions of the leading American astronomers on the "Universal Day."

In favour of the change being made on Jan. 1, 1885 :—Profs. O. Stone, H. A. Newton, M. W. Harrington, C. A. Young, Lewis Swift, S. P. Langley, J. G. Porter.

Unfavourable to the change :—Profs. Newcomb, E. S. Holden, C. H. F. Peters, H. S. Pritchett.

Prof. E. Pickering regards an agreement among astronomers as of more importance than any special mode of reckoning.

The Superintendent of the Naval Observatory, Washington, had issued an order directing the use of the Universal Day on and after Jan. 1, 1885 : but after a strong remonstrance from Prof.

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* Astron. Nachrichten, No. 2656.

Newcomb, and the above expression of opinion from the leading astronomers of the States, he withdrew the order, on the ground, as stated in the 'Observatory,' No. 96, p. 139, that the "weight of opinion" was against the introduction of the Universal Day at present.

ABNORMAL APPEARANCES OF THE SATELLITES OF JUPITER.—We would draw attention to Mr. Spitta's letter on this subject. We trust that observers of Jupiter will furnish him with the information he seeks, as the subject is well worth a thorough investigation.

REAR-ADMIRAL FRANKLIN, at present Superintendent of the Naval Observatory, Washington, has been ordered to take the command of the European Squadron.

OF the three minor planets recently discovered, No. 245 has received the name of Vera, and No. 247 that of Eukrate.

WE have received the third edition of Mr. Lynn's little work 'Celestial Motions.'

Maxima and Minima of Variable Stars in May 1885.

M, signifies maximum; *m*, minimum.

May	1	U Monocerotis, M.	May	19	T Canis Minoris, M.
	2	T Virginis, M.		22	L^2 Puppis, *m*.
	3·7	ζ Geminorum, M.		22	S Aquilæ, *m*.
	5	S Ceti, M.		23	U Monocerotis, *m*.
	6	S Vulpeculæ, M.		23·4	T Monocertis, M.
	8	R Arietis, *m*.		24·4	W Virginis, M.
	10	R Lyræ, *m*.		25	R Lyræ, M.
	11	R Scorpii, M.		25	R Orionis, M.
	15·5	T Monocerotis, *m*.		31·8	β Lyræ, *m*.
	16·2	W Virginis, *m*.			

Variables of Short Period.

δ Cephei.

	d.
May	4·25, *m*.
	9·65, *m*.
	11·25, M.
	16·65, M.
	20·35, *m*.
	25·75, *m*.
	27·35, M.

η Aquilæ.

	d.
May	5·5, M.
	10·3, *m*.
	12·7, M.
	17·5, *m*.
	19·9, M.
	24·7, *m*.
	31·8, *m*.

S Cancri, *m*.

		h	m
May	7	13	24
	26	12	39

δ Libræ, *m*.

		h	m
May	3	13	32
	10	13	6
	17	12	40
	24	12	14
	31	11	48

U Cephei, *m*.

		h	m
May	20	16	38
	25	16	18
	30	15	57

W Sagittarii.

	d.
May	3·5, *m*.
	6·7, M.
	14·3, M.
	18·7, *m*.
	26·3, *m*.
	29·4, M.

U Sagittarii.

	d.
May	16·6, *m*.
	19·6, M.
	23·4, *m*.
	26·3, M.

U Coronæ, *m*.

		h	m
May	5	13	3
	12	10	46
	19	8	28
	29	17	2

Approximate Times of Transit of the chief Markings on Jupiter in May and June 1885.

α. Red Spot.	h	m	β. Dark Spot.	h	m	γ. Equatorial White Spot.	h	m	δ. White Spot.	h	m
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
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[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	5..	9	[illegible]
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α. The red spot S. of the great southern belt.

β. A long black spot [illegible] in the narrow belt immediately [illegible] great S. belt. This spot maintains same rate as the red spot, and follows it 1h 45m.

γ. The old equatorial white spot. After some remarkable fluctuation of motion and brilliancy, this object appears to have maintained a fairly uniform rate, since Jan. 27, of 9h 50m 14s.34. It now precedes Mr. Marth's 1st meridian some 35m = 21½°.

δ. A large white spot indenting the N. side of the great northern equatoreal belt. It moves slightly swifter than the red spot, and is a very conspicuous object. On its *s.p.* side a vein of light runs from it into the dark belt, dividing it lengthwise, for some distance, and then emerges near the equator, where it is lost.

I have given the separate times of these objects, as they are each very conspicuous except the red spot; and as Jupiter is now nearing the end of the present opposition, it is very necessary these interesting features should be observed whenever it is found practicable.

W. F. Denning.

Bristol, 1885. April 18.

Astronomical Memoranda, May 1885.

Equation of Time:—Sun *before* Clock, May 1, $3^{\rm m}\ 4^{\rm s}$; May 11, $3^{\rm m}\ 50^{\rm s}$; May 21, $3^{\rm m}\ 38^{\rm s}$; May 31, $2^{\rm m}\ 34^{\rm s}$.

Sidereal Time at Mean Noon:—May 1, $2^{\rm h}\ 38^{\rm m}\ 19^{\rm s}$; May 11, $3^{\rm h}\ 17^{\rm m}\ 44^{\rm s}$; May 21, $3^{\rm h}\ 57^{\rm m}\ 10^{\rm s}$; May 31, $4^{\rm h}\ 36^{\rm m}\ 35^{\rm s}$.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk.	
				Lat.	Long.
	h m	h m	° ′	° ′	° ′
May 1	16 32	7 20	335 49	−3 55	195 24
6	16 23	7 29	336 50	3 23	129 17
11	16 15	7 36	338 1	2 50	63 11
16	16 7	7 44	339 23	2 16	357 3
21	16 1	7 51	340 54	1 41	290 54
26	15 55	7 57	342 34	1 5	224 45
31	15 51	8 3	344 23	−0 29	158 35

The Moon.

	rises.		*rises.*		*sets.*		*sets.*
	h m		h m		h m		h m
May 1	9 40	May 9	14 28	May 17	10 45	May 25	14 59
2	10 32	10	14 54	18	11 33	26	15 29
3	11 19	11	15 22	19	12 13	27	*rises*
4	12 1	12	15 52	20	12 47	28	7 31
5	12 36	13	*sets*	21	13 15	29	8 26
6	13 8	14	7 28	22	13 41	30	9 16
7	13 36	15	8 40	23	14 7	31	9 59
8	14 3	16	9 48	24	14 33		

May $6^{\rm d}\ 20^{\rm h}\ 43^{\rm m}$ Last Quarter. May $14^{\rm d}\ 3^{\rm h}\ 17^{\rm m}$ New Moon.
20 17 45 First Quarter. 28 8 31 Full Moon.

Occultations, May 1885.

Day.	Star.	Mag.	Disap.	Angle.	Reap.	Angle.
			h m	°	h m	°
1	29 Ophiuchi	6	15 14	52	16 22	301
16	130 Tauri	6	6 31	96	7 27	257
19	α Cancri	4	10 52	9	11 15	320
20	B.A.C. 3407	6	12 21	86	13 12	240
21	35 Sextantis	6	8 48	2	9 18	312
27	θ Libræ	4½	14 10	123	15 5	222

Planets for May 1885.

Day.	Planet.	R.A.	Dec.	Diam.	Rises.	Tr.	Sets.
		h m s	° ′	″	h m	h m	h m
1	Mercury..	2 14 13	13 23 N.	12·0	16 19	23 30	6 44
11		2 4 34	9 56 N.	10·8	15 49	22 43	5 38
21		2 20 29	10 17 N.	8·8	15 25	22 22	5 18
31		2 59 4	13 45 N.	7·2	15 7	22 23	5 37
1	Venus ..	2 32 58	14 9 N.	9·6	16 36	23 55	7 12
11		3 21 45	17 59 N.	9·6	16 21	0 4	7 45
21		4 12 23	21 2 N.	9·6	16 12	0 15	8 16
31		5 4 43	23 8 N.	9·6	16 12	0 28	8 42
1	Jupiter ..	9 54 33	13 58 N.	36·5	23 30	7 15	14 32
11		9 56 13	13 48 N.	35·5	23 18	6 37	13 53
21		9 58 58	13 32 N.	34·5	22 45	6 1	13 15
31		10 2 42	13 10 N.	33·4	22 11	5 25	12 37
1	Saturn ..	5 24 37	22 10 N.	15·3	18 37	2 46	10 53
31		5 40 6	22 24 N.	14·9	16 52	1 3	9 12
1	Uranus ..	11 58 44	0 58 N.	4·0	3 10	9 19	15 28
31		11 56 49	1 10 N.	3·8	1 10	7 19	13 28

May 4^{d} 5^{h}. Venus in superior conjunction with the Sun.
25 1 Mercury at greatest elongation (24° 45′ W.).

Phenomena of Jupiter's Satellites, May 1885.

d	h	m		d	h	m		d	h	m	
1	11	5	ii. Tr. I.	10	8	46	ii. Oc. D.	20	11	10	iii. Ec. D.
2	11	43	iii. Tr. E.	13	9	58	iv. Oc. D.	21	9	41	i. Tr. I.
3	11	35	ii. Ec. R.		10	25	i. Oc. D.		12	1	i. Tr. E.
5	11	24	i. Tr. I.		10	35	iii. Ec. R.	22	10	22	i. Ec. R.
6	8	32	i. Oc. D.	14	10	6	i. Tr. E.	26	11	6	ii. Tr. E.
	12	4	i. Ec. R.	17	11	21	ii. Oc. D.	27	9	57	iii. Oc. D.
9	11	56	iii. Tr. I.	20	9	33	iii. Oc. R.	28	11	37	i. Tr. I.

The R.A. and N.P.D. of Planets and the Solar Co-ordinates are given for Greenwich Mean Noon; in all cases Greenwich Mean Time is used, reckoning from noon to noon, as in previous years; the angles for Occultations are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

ERRATA IN No. 96.

Page 101, line 11 from bottom, *for* "requested to state" *read* "requested me to state."

Page 130, line 2 from bottom, *for* + *read* –.

Page 139, line 12 from bottom, *for* "appeared" *read* "appeared not."

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 98. JUNE. 1885.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, May 8, 1885.

E. Dunkin, F.R.S., *President*, in the Chair.

Secretaries: E. B. Knobel and
Lieut.-Col. G. L. Tupman.

The preliminary business of the Meeting concluded,—

Mr. Knobel read a paper by *Mr. R. A. Proctor* on "Five Orders of Meteor-streams." Mr. Proctor referred to the importance of Mr. Denning's discovery, which he supposed might be taken as now established, that certain meteor-streams radiate for months together from the same point. These meteors must travel at the rate of hundreds of miles per second, and they must look for the source of these bodies to the giant suns. He had formerly grouped meteor-streams under three orders. Mr. Denning's discovery obliged him to add two more. The five orders would be:—

I. Meteors ejected from our Earth when in sun-like state.
II. Meteors ejected from giant planets when in sun-like state.
III. Meteors ejected from our own Sun.
IV. Meteors ejected from suns of the same order as ours.
V. Meteors ejected from giant suns like Sirius, and imbued with enormous velocities.

Mr. Ranyard observed that meteors thrown out by the stars would, besides the velocity due to ejection, participate in the velocity due to the proper motion of the star, which might in some cases amount to some hundreds of miles per second. Still this would seem insufficient to account for the incredible velocities which Mr. Denning's observations appeared to indicate.

Col. Tupman. We must all admire the fertility of Mr. Proctor's speculations; but one serious objection occurs to me, viz. that meteors undoubtedly have not so great a velocity. I doubt whether any of them have a velocity of their own exceeding that of the Earth by half as much again.

Mr. Ranyard. Has Col. Tupman any evidence with regard to the velocity of meteors from any one of Mr. Denning's fixed radiant-points? No doubt there are many simultaneous observations, made by observers in different places, of bolides and large meteors which have enabled their velocity within the atmosphere to be calculated, and from theoretical considerations the velocity of meteors belonging to the planetary swarms cannot differ very much from parabolic velocities. But these swift meteors from fixed radiants must be consumed very high in the atmosphere, and I do not know what evidence there is to show that they cannot have these enormous velocities.

Col. Tupman. There is a certain amount of evidence to be deduced from comparing the angular velocities of meteors from different radiants. There are certain radiants from which slow-moving meteors come, and others from which the motions are more rapid; but there is nothing like the difference that would correspond to a velocity fifty times as great in one case as compared with another. At 90° from the radiant the average angular velocity appears to be always about the same for the majority of radiants. If there were any with double or treble velocities, it would be noticed at once by an experienced observer. At 90° distance from the radiant the speed is usually not too great to permit of your following the meteor with your eye. As a matter of fact, the velocity is essentially parabolic for all systems.

Mr. Mellor suggested that if the meteor was expelled from the rear of the star, with respect to its proper motion, it would have the motion of the star, *minus* the motion with which it was ejected, so that it did not follow that the meteor ejected would have the same motion as the star itself has.

Mr. Stone explained a paper on the Right Ascensions of the Cape Catalogues for 1850, 1860, and 1880. Mr. Downing, he said, had carefully compared the Cape Catalogue for 1880 with the Melbourne Catalogue for 1870, and as the epochs only differed by 10 years, such a comparison might give some evidence for or against the existence of systematic errors. The comparison instituted by Mr. Downing showed a close agreement between the R.A.'s of the two catalogues; but when attempts were made to compare the Cape Catalogues of 1850, 1860, and 1880, it became almost impossible to bring up the places for 20 or 30 years, with sufficient accuracy, to give trustworthy evidence of any differences in systematic errors. He had pointed out in the paper that the result of the comparison of 1850 with 1880 was that the R.A.'s of the latter appeared too small, while the comparison of 1860 with 1880 appeared to show that they were too large. It was quite certain that one must be wrong. He did not think the places of the 1850 Catalogue comparable in accuracy to those of the 1880 Catalogue, but the places of the 1860 Catalogue were certainly good. Taking out those stars which had a known proper motion, determined independently of the 1860 and 1880 Catalogues, and making allowance for it, he had reduced the difference to five-hundredths of a second in the one case, and three-hundredths in another comparison.

Mr. Downing was glad to find that Mr. Stone had been able to make some use of the comparisons of the Melbourne and Cape Catalogues. In one point he could hardly agree with Mr. Stone, that the comparisons were not trustworthy on account of the inaccuracy of the proper motions. In the comparison of the 1850 and 1880 Catalogues there were no less than 3368 stars used, and with such a large number as that, the errors of the assumed proper motions would hardly affect the result. When they took stars in zones extending over 5° of declination through the 24 hours, it could not be assumed that there was any decided drift. He had come to the conclusion, however, that the right ascensions of the Cape Catalogue for 1880 were superior to those of the Cape Catalogue for 1850, and in that he thought Mr. Stone would agree with him.

Mr. Stone replied that, of course, if they took a large number of proper motions, the errors might eliminate themselves, but he thought that would not apply to the cases he referred to. If any one would examine the proper motions in R.A. of southern stars, for the first four hours, they would find an extraordinary difference in the *plus* proper motions, and it would require a large number of observations indeed to effectually eliminate the differences, even if these did not at all depend upon imperfect corrections for precessional motions.

Mr. Knobel read a paper by *Mr. John Maguire* on "Total Solar Eclipses visible in the British Isles from 878 to 1724." The paper enumerated all those eclipses the central line of which had touched the British Isles, and gave tables showing the breadth of the shadows. London had been twice visited by total eclipses, viz. in 878 and 1715, Dublin twice, and Edinburgh five times. The Moon's shadow had fallen, during the periods named, on every spot in the British Isles, except a small place on the coast of Ireland.

Col. Tupman read a paper by *Mr. W. F. Denning* on "A Peculiar Variety of Meteors observed on the morning of April 20." As Mr. Denning was watching the progress of the Lyrids, he saw a remarkable meteor projected apparently on the stars ϵ and δ. It rose upwards with great rapidity. It was about the 3rd magnitude, but the chief peculiarity was its marvellous velocity. It appeared to be in the air only a few yards distant. It must have traversed over 16 degrees of the background of the sky in about ·05 second; but of course there was great difficulty in estimating such short intervals in the case of mere glimpses of pale white light. They had little analogy to ordinary shooting-stars, and suggested an electric origin.

The following papers were also announced:—

Rev. S. J. Johnson. "Observing Weather."

Lt.-Col. G. L. Tupman. "Observations of Comet 1884 II (Wolf) at Harrow."

N. E. Green. "Observations of Saturn."

E. J. Spitta. "A note of an Observation during the transit Jupiter's Satellite IV. April 18."

W. G. Thackeray. "On the Diameters of the Sun and Moo as observed with the Greenwich Transit-circle."

Dr. Gill. "Reply to Mr. Stone's paper on Screw-errors as affecting the N.P.D. of the Cape Catalogue for 1880."

Prof. C. H. Brewitt Taylor was duly elected a Fellow of the Society.

The Ordinary Meeting adjourned at $8^h\ 55^m$, and at $9^h\ 0^m$ the Special Meeting was commenced in accordance with the notice issued to the Fellows.

THE SPECIAL GENERAL MEETING.

Lt.-Col. Tupman read the notice convening the Meeting.

The Earl of Crawford, in moving the first resolution, said he would be as brief as possible. The proposal he had to bring forward was one which had engaged the attention of many of the Society for a number of years. He believed that many Fellows who did not live in or near London felt that they were under a certain disability in not being able to vote in the election of the Council and Officers and on the general conduct of the Society. Much might be said as to the desirability of the change proposed; but he thought they would give him credit for having only acted for the benefit of the Society at large, as he was not himself affected by the disability referred to. He desired that the Foreign Fellows, of whom there was a large number, should take a greater part in their affairs, and that the Society as a whole should be strengthened by the increased interest in it of all its members. The resolutions had been drawn up with care; but there was a verbal error in the first, which he would take leave to alter, as his so doing would not alter the sense. At present the resolution read "Lists handed," as if any Fellow might hand in more than one. He proposed to alter this to "A list." He would now formally move the first resolution. It was proposed to add the following words at the end of Bye-law 12:—"A list handed to the Scrutineers of the Ballot on behalf of any Fellow not present at the Meeting, shall be accepted, if duly verified by the signature of the absent Fellow."

Mr. Mattieu Williams seconded the motion.

Mr. Brett said that Members entered the Society well understanding that they were not to vote unless present at the Meeting. It was therefore hardly correct to say they were disqualified.

Lord Crawford. Debarred, then.

The President. Before the discussion proceeds any further, I think it may be as well for me, as President of the Society, to explain the action the Council have taken with regard to this question. The Council generally have great sympathy with the object of this motion; and they are desirous that something to the same general effect should be passed by you to night. (Hear, hear.) At the same time, they think certain precautions should be taken, so as to prevent any irregularity in voting. (Hear, hear.) It is their view, therefore, that the list should be forwarded by post in a closed envelope, under cover of a letter signed by the Fellow and addressed to the Scrutineers of the Ballot, and that such list should be accepted by the Scrutineers. There can be no objection whatever to an absent Fellow voting in that manner. But it would not, in my opinion, be well that country Fellows should send their lists to other Fellows, and that the latter should present them to the Scrutineers. I have had some forty years' experience in this Society; and you will allow me to say that I believe we shall be doing the Society great harm unless we add those precautions of which I have spoken. The Council have every sympathy with the intention of the present motion; and if Lord Crawford will accept the remarks I have made, I think the resolution might be almost passed unanimously. (Hear, hear.)

Lord Crawford asked if he understood the President's proposal rightly. It was practically this—that the Scrutineers would be entitled to receive from any Fellow a list conveying his desires with reference to the election of the Council, provided that the list was enclosed within a letter stating that desire, and received by the Scrutineers through the post.

The President. Yes, that is so, after identification.

Mr. Chambers believed that a large number of the Fellows of the Society would welcome the concession which had been made by the President on behalf of the Council. At the same time, he hoped that Lord Crawford would not withdraw his motion, except on a very clear understanding that the Council would pledge itself to propose, at an early meeting of the Society, such an amended resolution as had been suggested. At present the great bulk of the Fellows were practically disfranchised. There were 500 Fellows of the Society; and instead of the great bulk taking part in the election of the Council, it practically amounted to 20 Fellows joining with the Council to elect 20 who had already been selected by the old Council. The result was that the Council of the Society did not command the unreserved confidence which it otherwise would. In evidence of that he would point to the fact that the Society did not now attract to itself the important papers which it attracted fifteen or twenty years ago. (Question.)

Mr. Beck hoped that the change proposed would not be made. He felt strongly as regards the independence of the Fellows of the Society; and this change would be throwing into the hands of the Council a power which it was not desirable to give. It might sometimes happen that it was desirable to remove a member from the Council; but if the proposal was adopted, there was no doubt the majority of the country members when receiving those lists, and not knowing the circumstances of the case, would vote for the Council's list: and there would therefore be a greater difficulty in making any alteration from the whole list than there was at present. He believed they would be entirely giving up their freedom of introducing a new name if their votes were to be overwhelmed by those of a large number of persons knowing nothing of the detailed circumstances of the case. He believed his views would be sympathized in by a large number of Fellows who attended the Meetings, and he hoped no change would be made.

Sir Edmund Beckett said the proposal of the President seemed to him to be a very good one. It was merely a matter of machinery as to how absent members should be allowed to register their vote. He could hardly understand how there could be any dissentients to the proposition. It should be remembered that in voting for the Council there was never any debate, and there could be no debate, about a person's character. He could see no reason against the proposition, except the jealousy of those who lived in London against those who did not live in London. He thought there was force in what had been said about the greater interest that would be taken in the affairs of the Society by absent Fellows, and he feared that he must endorse what Mr. Chambers had said about the evident decline in the interest that was taken in the affairs of the Society compared with what used to be. This was a practical reason why, if it was wished to keep the Society flourishing, they should pass this resolution.

Mr. Ranyard considered that before the motion was adopted they should have the words the President wished to substitute stated very definitely.

The President. A list sent by post in a closed envelope, under cover of a letter signed by the Fellow and addressed to the Scrutineers of the Ballot, shall be accepted as the voting-paper of that Fellow.

Mr. Ranyard thought that the less the original motion was changed the better. The list should be verified, and there should be no chance of any member handing in more than one. He thought that no great harm would be done by substituting the word "posted" for the word "handed" "to the Scrutineers," though he did not see that much would be gained by it in practice, for any country Fellow would be able to send his balloting-paper to any one in London who undertook to canvass for votes, and the London Fellow might then post the lists to the Scrutineers instead of handing them, and he saw no advantage in making the country Fellow sign the outside of an envelope rather than sign the balloting-list itself. It was perfectly impossible to prevent the Scrutineers knowing a person's vote, if they thought fit.

Sir Edmund Beckett. You mean to preserve the right of people to hand in their own papers when they are here?

Lord Crawford. Yes.

Mr. Newall lived 300 miles from London, and was deprived of voting, when he should like to do so, owing to the distance he lived away. He saw no objection to a voting-paper being sent in an envelope, with the signature of the Fellow outside, and the Scrutineers could open it and throw the paper into the basket, and no one need know how a vote was given.

Captain Noble said, as to London Fellows voting, he wished to remark that he occasionally attended scientific meetings of the Society, and met a number of gentlemen, but when he came to the three-o'clock meeting he found a totally different set of people. He thought it advisable that people who attended the business meetings only of the Society should not be allowed to outvote those who attended the ordinary scientific meetings. He considered that the proposed alteration would put everybody on the same footing, and he hoped that it would be adopted.

Prof. Schuster thought that no change should be made in the voting at present. The form of the present proposal was only a little less objectionable than the first proposition; and he thought they ought not to accept it except under the distinct understanding that the second paragraph be withdrawn; otherwise, if they admitted voting by proxy on one question they might admit it on all questions. The remark of Capt. Noble would lead them to the conclusion, which he thought was a reasonable one, that it would be well to alter the time of the Annual Meeting, and have it in the evening instead of in the afternoon. One objection he had against the resolution was, that it would admit the principle, which certainly ought not to be adopted, namely, that of allowing a man to render a vote without having heard the arguments on each side. Members who wished to vote could easily get to London. Only that day he had delivered three lectures in Manchester, and his last one he finished at four o'clock, and he then left to attend the meeting. Fellows present should not be led away by a mistaken notion of kindness to those who were absent.

Mr. Stone's sympathies, so far as he was individually concerned, were entirely in favour of granting the power of election to the Fellows generally. He could not see that the fact of a man coming to the meeting and giving his vote in person gave that vote any more value than if he had sent the list from his home by post. The two propositions were practically the same, the only difference being that the one brought forward by the President took certain precautions. It was necessary that a Fellow should send a letter stating that the voting-paper enclosed in the envelope was his. The Scrutineers would open the envelope, and be able to tick off his name as having voted, so that he could not attend the meeting and vote again. His paper would be put into the box just as if he were in the room and put it in himself. He saw nothing revolutionary at all in that idea. He, however, held a very different opinion with regard to the second resolution. In fact, he was in favour of the first because he was opposed to the second. He wished the affairs of the Society to be managed by the Council, and he wished the Council to be in sympathy with the Society at large. (Hear, hear.)

Mr. Knobel apprehended that the motion had been brought forward under the idea that a grievance existed. He believed that a grievance did exist, but it was due solely to the machinery of the Bye-laws. He thought it would be an advantage if every Fellow belonging to the Society could vote for the Officers and Council. The suggestion in the motion before the meeting that the voting-paper should be signed was a distinct departure from other Bye-laws, which require that the election should be by ballot, that is, by secret vote. The suggestion which had been made by the Council would get over any difficulty the original motion might raise, and he should be very glad if that suggestion could be accepted unanimously by the meeting.

Lord Crawford thought it would be very desirable for them to accept the principle which the President had laid down. He would therefore propose that the first resolution should read as follows:—"Any list addressed and posted to the Scrutineers of the Ballot, on behalf of any Fellow not present at the meeting, shall be accepted, if duly verified by the signature of the absent Fellow on the envelope."

Mr. Mattieu Williams said that, as seconder of the motion, he would accept the modification.

Mr. Brett denied that any one was jealous of the country members; but it seemed to him that if the resolution was passed the control and appointment of the officers of the Society would be in the hands of touts and wire-pullers.

Mr. Bryant thought that some time for sending in the lists should be fixed, otherwise the Scrutineers would not have an opportunity of verifying them if they came late.

The President thought they should be sent in by 12 o'clock on the day of the meeting.

The resolution, as modified by the President and accepted by Lord Crawford, was then put to the meeting and carried with only six dissentients.

Mr. Knobel called attention to Bye-law 53, and said that it only applied to sending notices to Fellows in or near London.

Mr. Ranyard said that practically notice was given to all members in England and abroad.

Lord Crawford, in moving the second resolution, urged that what he had said before as to the feeling of members of the Society residing at a distance, with regard to voting for the election of the Council, applied in every way and in the same manner in regard to the voting on subjects which were brought forward at special or annual meetings. He should be against the principle of having a proxy of any description and for anything; it ought to be for a definite object. For instance there were many cases in which proxies holding good for twelve months were legally handed from one person to another. He should consider that would be highly inadvisable to have in a Society like this; but where the subject was thoroughly well known before hand, he saw no objection to it. He concluded by moving:—

"That Fellows of the Society may vote on the business brought before the Annual General Meeting or Special General Meetings either personally or by proxy."

Mr. Newall seconded the motion.

Mr. Turner said, that as absent country members would not hear the arguments on the various questions which arose, he should not be inclined to let them participate in deciding them.

Mr. Chambers was in favour of this resolution.

Mr. Stone said that however much he was in favour of the first resolution, he was equally opposed to the present one. It was extremely undesirable that there should be appeals from the Council to the general meetings. If they had no confidence in the Council, if the Fellows were not properly represented upon it, by all means let them elect other men in whom they would have confidence. There might be disagreements on the Council, and a member might appeal to the general meeting against the rest of the Council. In order to succeed he would probably tout for proxies, and the Council would have to do the same if it wished to defeat him. He could not conceive anything more deplorable than a meeting of this Society being called where no heed whatever was taken of the arguments on the two sides, but the question simply depended upon which member had secured the largest number of proxies.

Sir Edmund Beckett was sorry to say Mr. Stone had not convinced him to his side, but had convinced him the other way. (Laughter.) On the same principle that he voted for the former resolution he should, after Mr. Stone's speech, vote for this.

Capt. Noble, as a country member, said he should only be too glad to be able to express his opinion without having to come up to London to do it.

Admiral Ommanney asked how many members must be present to constitute a general meeting.

Mr. Knobel said the bye-laws did not state the number of members necessary to form a quorum at the annual meeting; but it was necessary to have 12 at ordinary meetings.

Prof. Schuster felt very strongly on this matter. The only argument brought forward in favour of the resolution was, that men were not influenced by discussions. If once that principle was admitted, he thought they should propose to go one step further and say they would take the voting first and the discussion afterwards.

Mr. Ranyard said if a member chose to vote without hearing what might be said on either side, he did not see why he should not do so.

The Resolution was then put to the meeting and lost, 35 voting for, and 38 against it.

The President. With regard to the post-cards sent out by order of a resolution passed at the Annual General Meeting, requesting Fellows to say whether they desired a change in the hour of meeting or not, I have officially to report the following result:—Those in favour of the meetings continuing to be held at 8 o'clock, 106; those for meeting at 5 o'clock, 125; neutral, 150, and other hours, 17; so that you see there is no decided preference.

Mr. Chambers moved "That in accordance with those figures, it is expedient that the hour of meeting should be 5 o'clock. He complained that the word "neutral" had been put on the post-card instead of simply "8 o'clock" and "5 o'clock."

Mr. Waters seconded the motion.

Mr. Ranyard thought it was understood at the general meeting that unless there was a very decided majority in favour of a change, nothing should be done. There did not seem to be a decided majority.

Mr. Knobel said that if the meeting was to be held at 5 o'clock, the Council would have to hurry over a great deal of important business.

Lord Crawford said that had the question been definitely put 5 o'clock or 8 o'clock, he should have been for 8 o'clock; but it occurred to him that 7 o'clock would have been more convenient, as a great deal of important business was now not done for want of time.

The Resolution, proposing that the ordinary meetings be held at 5 o'clock, was then put and lost.

The proceedings then terminated at 10.30 P.M.

The Liverpool Astronomical Society.

THE closing meeting of the Fourth Session was held on Monday, May 11, the Rev. T. E. Espin, B.A. (President), occupying the Chair. In a paper on the possible origin of comets, Mr. J. R. Sutton said the recent return of Encke's and Tempel's Comets afforded an opportunity of considering what might have been their origin. The periodic time of Encke's Comet had been observed to decrease by an average of about two hours in each revolution, and there could be no doubt that this would eventually land it on the Sun's surface. The most natural explanation of this peculiarity was that some retarding influence gave the Sun's attraction greater power than would otherwise be the case. The theory that comets had been erupted from the interior of planets was very ably argued by Mr. Proctor; but the only factors that could support such an hypothesis seemed to be that the aphelia of periodical comets crowd upon the orbit of Jupiter, and that the ascending node of Tempel's Comet was very near to the orbit of Uranus. But the connection which we now knew to exist in so many instances between comets and meteor-systems was fatal to Mr. Proctor's view, because if Tempel's Comet were erupted from Uranus, so also must have been the meteor-system to which it belonged. But he contended that no planet could by eruption generate a quantity of matter which would revolve in a continuous ring around the Sun. Whether they came to us from the stars might reasonably form a matter for dispute, though spectroscopic analysis certainly seemed to point in that direction. But it was certain that a retardation had been proved sufficient to render either the planetary capture or eruptive theories unnecessary for the argument. Mr. W. F. Denning reminded Mr. Sutton that there were only four instances of certainly known cases of identity between cometary and meteoric orbits. The connection was proved rather by the preciseness of such agreements than by their number. He thought, also, it was quite possible that comets might have a derivative source in planetary eruptions. A planet might, for example, have evolved a certain quantity of matter in the form of an immense cloud of meteoric pellets, or, say, a volcanic outburst similar to the great Java eruption of August 1883, and this cloud would, if ejected with sufficient force to leave its parent planet, certainly tend towards the Sun, and in process of time assume the form of a meteor-ring. The foremost particles of the original mass would primarily gain ground on the central portion, and would thus cause an extension of the stream. Then, as the mass passed perihelion, there would naturally be a slight difference in perihelion distance, and this meant a difference in periodic time. Granting therefore that comets and meteors had been erupted from planets, their subsequent resolution into rings could only be a question of time.

Mr. Joseph Baxendell said Capt. Noble had lately drawn the attention of the Royal Astronomical Society to an apparent discrepancy between the proportions of Saturn and his rings as seen in a telescope, and those derived from data given in the N. A. Since the publication of Capt. Noble's communication it had struck him that a phenomenon he had witnessed in Oct. 1882 strongly supported the position Capt. Noble had taken. At the date mentioned the minor axis of the outer ring was, according to the N. A. elements, less than the polar diameter of the ball; but he had observed the S. limb of the planet to extend sensibly beyond the southern edge of the outer ring, though no trace of the N. limb could be perceived on the northern edge of the ring. This appearance became well-marked on October 28, when it was readily seen with a power of 180. As the elevation of the Sun above the plane of the ring was at this time about 11½ minutes less than that of the Earth, the invisibility of the northern limb of the planet could not have arisen from the shadow of the ring being thrown upon it. Assuming the elements in the N. A. to be correct, the only explanation appeared to be that the centre of the ball was not coincident with the centre of the ring-system, but was at a sensible distance on the S. side.

Mr. R. C. Johnson read a paper by Mr. Geo. Knott, on the variable U Geminorum. Circumstances seldom permitted a complete observation of a maximum of this star. In addition to cloudy skies, a further disturbing cause was to be found in the Moon, whose path was comparatively near the star's place, and whose presence sometimes seriously interfered with, if it did not entirely prevent, all effective observations. The brightness of the star had been observed to vary from 14·5 m. on April 1 to 9·5 m. on the 6th, when it slightly decreased in brightness, and reached its principal maximum of 9·2 m. on the 8th. It would therefore appear that on this occasion the maximum was a double one, with a subsidiary minimum intervening—a phenomenon which had been previously observed in this star. Professor Schönfeld had pointed out in his 'Zweiter Catalog' that not only were the intervals between successive maxima subject to great irregularity, but the duration of the maximum phase (or length of time taken in rising to maximum and falling to minimum again) had a varying range of from ten to twenty days. It would seem indeed that the maximum might be broadly divided into two types—one well marked with a duration time of 10 + days, the other (not perhaps presenting quite such distinct characteristics, but still well marked) with a range of from fifteen to twenty days. The longer maximum was sometimes marked by a slight subsidiary minimum. Whether the maximum was of longer or shorter duration the rise appeared to be rapid. He had not found any regularity of sequence as to long or short maxima, nor did there appear to be any decided relation between successive maxima and the lengths of the duration. Neither, on the other hand, did it appear that the point of magnitude touched was in either case higher or lower than in the other. But at the same time it must be admitted that the observations were hardly sufficient to enable one to speak decidedly on points of this nicety.

The President read a paper on the probable period of the variable star LL 14551. This star had been observed to be variable by him in 1883, and photometric observations of it were published in the 'Monthly Notices' of the R.A.S. vol. xliii. p. 432. During 1884 it had been observed with a photometer of his own contrivance, but only very casually, as the star was badly situated in our English sky. Mr. W. E. Jackson of Constantinople had, however, kindly taken up the observations, and, as an example of the magnificent opportunities enjoyed by astronomers in that part of the world, no less than sixteen complete observations were obtained last month, eight of them being on consecutive nights. From these observations a period of 14·09 days was obtained. The photometric observations of 1883 gave a similar period. The variation in light was very irregular. After a maximum the light of the star decreased for 1½ days, it then increased for about the same period, and, after remaining stationary for about two days, showed again a tendency to increase for 1½ days, after which it diminished and then remained stationary, rising steadily for 2½ days before the maximum. As this was the first variable star known in this constellation, he purposed to call it R Puppis.

The Secretary read a paper by Mr. J. Baxendell, Jun., on transits of Jupiter's fourth satellite. Attention had lately been drawn to the appearance of IV. as a dark spot when in transit, and it might be worth while to submit a series of his observations of this phenomenon made with a 6-in. refractor. On May 18, 1884, at 9.14, IV. and also the shadows of I. and II. were on the disk; IV. appeared as a dark spot, almost like a shadow, and nearly, if not quite, as dark as the shadow of I. The shadow of II. was not so dark, but more like Indian ink, and not more than one third of the diameter of the shadow of I. On Feb 27 last, at 10.15, IV. appeared as a very black spot, much darker and better defined than the shadow of I. On April 18, at 7.50, IV. was observed not far from the *f* limb, but not nearly so black as on previous occasions; though two hours later it was seen to be much darker: at 11.15, when it was very near the *p* limb, it was scarcely to be distinguished from the planet. Altogether the observation seemed to confirm the statement lately made by Mr. Knobel, that IV. appeared much darker in the middle of transit than when near either limb of the planet.

In a paper on the formation of lunar bays, Mr. S. E. Peal called attention to the shapes of Le Monnier and Fracastorius, which, he thought, seemed to show that they were the remains of circles once complete, and now probably deformed by some influence of the adjoining *Mare*, which had bodily removed the plain from that side. Those who held that the bays were ruins of complete circles were, however, met by two insoluble difficulties: 1, how such a huge circle could have been formed; 2, how so large a segment could have been so completely removed as to leave not a trace behind, which, on the volcanic hypothesis, appeared to be impossible.

In submitting a voluminous report of the planetary section, Mr. W. S. Franks said it contained a summary of the work done by members of the section during the recent opposition of Jupiter. No fewer than 185 sketches, many coloured, had been received, and some of them were exceedingly well done. The remains of the great red spot had received considerable attention, and dark transits of III. and IV. had been repeatedly noted.

Other papers were read, and a motion to enable members residing at a distance to vote for the election of the Council was unanimously agreed to. Seven gentlemen were elected members, and Messrs. W. S. Franks and J. E. Gore were elected Associates of the Society.

The Defining Powers of Telescopes.

My notes in the March number of the 'Observatory' have been fortunate in evoking comment from authorities of such experience and position as Professors Hall and Young and Mr. Green.

[illegible] are [illegible] minimised by prejudice, the evident [illegible] to [illegible] discuss the question on its merits.

I [illegible] the conclusion that what has been said [illegible] Mr. Green's 18-inch [illegible] the category [illegible] certainly [illegible] from a [illegible] refractor. [illegible] that a telescope having [illegible] in this manner is no longer eligible in [illegible] aperture. In effect [illegible] a smaller instrument, the [illegible] and atmospheric [illegible] with the degree of con- [illegible] in judging the merits of a telescope of a [illegible] diameter ought to be always understood [illegible]. If a stop of 15 inches [illegible] then the results are only in perfect [illegible] instrument of 15 inches aperture. I am afraid that many large telescopes gain credit for efficient performance not really possessed, the extreme diameter being greatly reduced though the fact is not mentioned.

Mr. Green admits that notwithstanding the excellent figure of his speculum from centre to margin, he finds it imperative to employ a [illegible] gradation of stops in order to improve its efficiency by rendering it a smaller instrument! If a large diamete be useless without stops, wherein does its utility consist? Bette at once adopt a smaller speculum and obviate the more troublesom management of a large instrument. True there are very rar occasions when all the aperture may be utilized: but are they wort waiting for, and then [illegible] do the results answer expectation

Prof. Young's comparison of the 23-inch Princeton refracto with a 9½-inch lens is very interesting. I am rather surprised a the advantage which he claims for the larger instrument on al occasions. On a bad night I should have supposed the 9½-inch would give tolerable views while the 23-inch would have been utterly useless. If the lesser telescope is a thoroughly good one it ought to show the red ellipse, now forming the visible remains of the red spot, with conspicuous plainness, as this feature is easily seen on a good night in a 5½-inch refractor. Of course separating power is a function of aperture, and the expansion of a very minute planetary detail can only be effected by the high powers which may be used with large instruments. But if from optical necessity the capacity of the 9½-inch is restricted to 0″·4, how is that the dark lines on Mars observed by Schiaparelli in February 1882 (with a power of 417 on the Merz refractor of 8 French inches aperture at the Milan Observatory) were only some 0″·15 in width, probably less, and yet "the greater number of the canals and their pairs were observed with comparative ease whenever the air was still?"

If Prof. Young finds it feasible to critically examine the faint details on Jupiter with powers of 950 and 1200, his telescope must be a very remarkable one. Certainly no small instrument can boast a record anything like this. Jupiter is rather a test of definition, and I had no idea there was any telescope in existence which would give us a sharp disk and a steady view of the more delicate features with a power of 600, or only half that mentioned by Prof. Young. When, however, your able correspondent states that he has successfully employed 1200 to settle critical matters of detail on the planet, I have only to confess that the Princeton refractor has effected a very fine performance and one which speaks volumes for its defining capacity, and for the favourable site of the observatory as regards meteorological conditions. The latter opinion is strengthened by the fact that a few years ago the 9½-inch refractor of that observatory showed the two satellites of Mars, the two outer satellites of Uranus, and the inner satellite (Mimas) of Saturn. It would be interesting to learn whether Prof. Young employs stops in his large instrument and how often he finds the full aperture available for work on bright planets. Also what powers are usually most efficient, and how often during the year is the seeing so good that the performance of the 23-inch is "immensely better" than that of the 9½-inch. His experience with a large telescope is of so exceptional a character that any further information he may afford us will be of deep interest. It now remains for the Princeton refractor to further exhibit its powers by grasping new features of planetary phenomena, for after all we must look to original results as the most safe and enduring proofs of real capacity.

Many of us will regret Prof. Hall's thoroughly discouraging report of the 26-inch refractor at Washington. No doubt it will be in part explained by unfavourable atmosphere. This instrument came from the same workshop as that at Princeton, and there seems no reason to doubt its equal merit on all points of its construction. In definition there would, however, appear to be a vast difference. During the observations of the markings on Mars made during August to October 1877 with the Washington telescope, "eyepieces magnifying up to 400 diameters were tried each night, but it was usually found that a power of 175 gave the best result" ('Monthly Notices,' xl. p. 13). Here is a great contrast to the powers of 950 and 1200 employed on Jupiter at Princeton! Yet it is a matter of common experience that Jupiter will not bear high magnifiers as well as either Mars or Saturn. The experience of Prof. Hall (expressed in his note with all that freedom and disinterestedness which belong to the true scientist) as to the non-utility of his large instrument in displaying planetary detail is quite in accord with nearly all others having similar instruments under their direction. It would be important if the manifest differences in the two telescopes we have been alluding to could be sought out. The comparison of meteorological records at the two stations might suggest a clue to the difficulty.

It is year by year becoming a matter of more serious consideration as to the most desirable sites for really large apertures, the uselessness of mounting them in situations where their immense resources cannot be fairly utilized being now fully recognized.

Returning to the comparison of large and small telescopes, of course there are times now and then when the seeing is magnificent, and these may occur with thin cloud or haze dimming a planet so much that an unusual grasp of light is indispensable. Again, when the air is very transparent and the image sharp, a large instrument may also possess an advantage, because a greater magnifying-power is then available. But these cases are exceptional. Sir. W. Herschel mentions one in the Phil. Trans. vol. lxxxv. as follows:—"Aug. 28, 1789, having brought the 40 feet to the parallel of Saturn I discovered a sixth satellite of that planet and also saw the spots upon Saturn better than I had ever seen them before." He remarks also at a later period that the 6th and 7th satellites were not discernible in the 7 or 10 feet reflectors, though "all that magnifying-power can do may be done as well with the 7 feet [of $6\frac{3}{10}$ inches aperture] as with any larger instrument." This remark in conjunction with that quoted in my paper printed in the 'Observatory' for March, p. 80, "having already found that with this instrument [the 7-feet reflector] I had light sufficient to see the belts of Saturn completely well, and that here the maximum of distinctness might be much easier obtained than where large apertures are concerned," render it clear that this great observer valued his small mirrors to an extreme degree, and it is well known he took immense pains to work them as near theoretical perfection as possible. They were his favourite appliances because he found them more practically serviceable than larger sizes. Of course many improvements have been effected since his day, but the principles of telescopic construction and the impediments to observation are much the same, and I think we may do well to ponder over the experience and suggestions of some of the old masters, whose methods and ideas have not been rendered obsolete by all that modern art has attained or observational discovery perfected.

My contention is that the superior quality of planetary images formed by object-glasses or specula ranging from about 6 to 10 inches aperture is usually quite a set off against the greater command of light obtained by large instruments. Taking the average performance there will remain no advantage on the side of the larger aperture, unless the case is special, as that of the Princeton Observatory. As a really efficient tool for systematic work on planets, telescopes of about 8 inches aperture cannot be surpassed. It is useless waiting for the two or three serene nights in a year when the whole diameter of a big instrument is available with really good effect. Amateurs urgently require the appliances most efficient under ordinary conditions, and they will find a large aperture of little avail until it is much reduced by a system of gagging and robbed of that very advantage which is extolled so much—namely, grasp of light.

The 18½-inch equatoreal of the Dearborn Observatory cost about £3700, the great Washington refractor cost £9000, the Melbourne Cassegrainean refractor of 4 feet aperture cost £14,600, and at first it would appear preposterous that a good 8½-inch With or Calver mirror, that may be purchased for some £30, will effectively rival these expensive and elaborate instruments. Many people would consider that in any crucial tests the smaller instrument must be utterly snuffed out; but such an idea is entirely erroneous. What the minor telescope lacks in point of light it gains in definition. When the seeing is good in a large aperture it is superlative in a small one. When unusually high powers may be employed on the former, far higher ones proportionately may be used with the latter. We naturally expect that very fine telescopes, upon which so much labour and expense have been lavished, should display far more detail than moderate apertures; but when we come to analyze the results it is obvious such an anticipation is far from being realized. The glare of excessive light and the endless mouldings and flaring of the image can only have one effect in obliterating delicate markings. It may sound imposing to describe, as has been done, the advent of Jupiter into the field of a very large reflector as "like a coach-lamp," or the approach of Sirius near the field of a similar instrument as "like the dawn of morning," and its real ingress as accompanied "with all the splendour of the rising sun"*. But I think there is something more attractive in the pale disk of a planet as it comes unannounced into the field of a small aperture and displays a perfectly still, sharply cut outline, enabling the most minute detail to come steadily out with all the exquisite clearness of an engraving. Such views as these cause us to linger for hours at the telescope, and to value moderate instruments for their really great, though unpretending, capacity.

W. F. Denning.

The Markings on Jupiter.

The details of this planet have been particularly interesting lately, in consequence of the variety, definiteness, and persistence of their forms. The old red spot has recovered much of its former character; it is readily seen, and under average definition its outline can be traced entire; the following end is very definite, and the white cloud in the interior is by no means so clearly seen as during the early portion of the opposition. The most interesting mark is one that follows it, in the same latitude, and is on the meridian about $1^h\ 40^m$ after the old red; this is the black spot of

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* I do not quote this with any attempt at ridicule, but rather to suggest that something else is required than mere excessive glare of light. The *simile* itself I cannot but regard as most apt, and it was forcibly recalled to me when, after some hours' comet-seeking one morning in October 1881, the star Arcturus approached and then entered the field of my 10 in. reflector. After the eye has become accustomed, during some hours, to faint objects passing in review it is remarkable how vivid an impression a brilliant star makes upon the retina.

Mr. Denning's list, to whom we are much indebted for his approximate times of transit. My first drawing of it was made on Feb. 12, when it figured as a dark line on the polar edge of the great southern belt. By the 12th of March it had so much increased in size and become so definite that a memo. was made on the side of drawing, "Curious dark mark." On the 15th, a night of very fine definition, two additional marks of the same character were seen; the first was very minute, the other nearly half the size of the one following the old red spot. These three dark marks followed each other in the same latitude and could all be seen on the disk at the same time.

By April 18 the first of these markings had become definitely oval, and its colour was noted as unmistakably red, not so red as the old red spot at its best, but as red as the edge of the southern belt. I have not observed the colour so marked since, but it can still be seen on a good evening. Altogether 31 drawings of these forms have been made, and I regret that the opportunity of observing them will be so soon past. The first and last are still well marked, but the small intermediate one was only seen on the night of the 15th of March.

It will be very interesting to see if they outlive the coming conjunction; if so, they may throw some light on the formation of the old red spot, which seems to have escaped observation.

NATH. E. GREEN.

39 Circus Road, St. John's Wood.

Astronomical Notes.

COMET II. 1867 (TEMPEL'S).—An exhaustive search has been made for this comet with both the 5- and the 6-inch refractors, the region for some 5° or 6° round the calculated place having been swept over about a dozen times during the absence of the Moon in April. A great number of nebulæ were encountered in the search.

NEBULÆ.—G. C. 1589. The 8 m. or 9 m. star is in the *n.p.* border of this nebula; not in the middle, as recorded in the General Catalogue.

G. C. 4036. Not "ee F." It is very small, pretty bright; resembling with low powers a small bright star slightly out of focus: it is visible in the 1½-in. finder. Its description in 1884 with the 6 in. refractor was S; PB; v S v m b M. The place does not quite agree with the General Catalogue.

G. C. 5315 (Supplement). Winnecke's description is erroneous; the place also is slightly out. It is an 8 m. star in a bright extended nebulosity; the star is not central, but slightly south of the centre. The nebula is a wonderful object; see Prof. Swift's notice of it in the 'Sidereal Messenger' for March 1885. The nebulosity is extended nearly north and south. The involved star forms a large nearly right-angled triangle with two stars equal in magnitude to each other, but slightly brighter than the involved star.

Dark Transit of Jupiter's Fourth Satellite.—This satellite was observed in transit as a very black spot on the evening of Feb. 27. I estimated it to be about one half the size of its shadow, which was following the satellite. The latter remained black until emergence, when it was seen dark up to the very limb of Jupiter. After emergence it appeared under a low power (60) as bright as any part of Jupiter; but when a high power (200) was used it seemed to fade very much, becoming dim with the increase of power, far more so than either of the other satellites.

Spots on Jupiter.—I have lately paid some attention to Jupiter with the 6-in. Cook equatoreal.

April 20. A sharp indentation was visible in the outer edge of the N. belt; this was a very marked and peculiar feature. I estimated that it was on the central meridian at $8^{h}\ 2^{m}{\cdot}5$. During the same observation a white spot in the S. equatoreal belt transited at $10^{h}\ 1^{m}{\cdot}5$.

April 21. At $10^{h}\ 32^{m}$ the great red spot was noticed some distance past transit. It was very pale; the following end was well seen. The southern edge of the equatoreal belt, following the spot, extended some distance southwards.

April 22. Notch in north edge of N. equatoreal belt on the central meridian at $6^{h}\ 56^{m}$. The preceding edge of this notch projects out conspicuously. A small round black spot, like the shadow of a satellite, difficult to see but remarkably well defined, on the southern edge of south equatoreal belt on the central meridian at $7^{h}\ 5^{m}{\cdot}5$.

April 24. The notch in north edge of equatoreal belt transited at $8^{h}\ 38^{m}$. At $8^{h}\ 51^{m}$ the small black spot noticed on April 22 was on the central meridian. It is a remarkable object, seemingly round and as well defined as a satellite's shadow, seen only with some trouble. At $9^{h}\ 4^{m}$ a small oblong dark spot, midway between south edge of the equatoreal belt and south pole, transited the central meridian.

April 25. At $7^{h}\ 8^{m}$ a large luminous ill-defined spot on the north edge of N. equatoreal belt was in transit. This is a very peculiar object and makes a dent or notch in the outer (northern) edge of belt. At $7^{h}\ 56^{m}$ a small white spot on the inner edge of the south equatoreal belt was in transit; the spot was small, white, and well-defined. The great red spot was observed to-night. It seems to have about the same size and form as in previous years, but it is only the ghost of the red spot now! It is very very pale, with a feeble pinkish tint. When a steady view was obtained it seemed to be clearly outlined and to be not diffused at its edges. There was a mass of darkish shading *s.p.*, possibly in contact with the south edge of the spot. The equatoreal belt curves southward following the spot. I estimated that the red spot was on the central meridian at $10^{h}\ 41^{m}{\cdot}5$. A large darkish mass was visible in the equatoreal belt north of the red spot.

The times given are Nashville mean time throughout. The longitude of the observatory is $5^{h}\ 47^{m}\ 13^{s}$ West of Greenwich.

Errata in No. 96.—In my contribution to the 'Observatory' for April, No. 96, p. 123, under the head "New Nebulæ," the following corrections should be made. In the *descriptions* interchange *a* and *b* to correspond with the positions given. Thus for "(*a*) small, round, &c.," read "(*b*) small, round, &c." And for "(*b*) very faint, &c.," read "(*a*) very faint, &c." Under the head "Two small Red Stars," p. 124, for the epochs of the places of the stars read 1884·0 instead of 1885·0, in both cases.

E. E. Barnard.

Vanderbilt University Observatory,
Nashville, Tennessee, 1885, April 25.

Parallax—Solar and Stellar.

The article on "Parallax" in the ninth edition of the 'Encyclopædia Britannica,' now in course of publication, has been written by Dr. Gill, than whom there is no astronomer of the day more qualified to write on this subject, into the investigation of which he has thrown himself with such energy and success. By far the larger part of the article is occupied (as is fitting) with the question of solar parallax. Dr. Gill explains fully and well the three distinctive typical methods which have been employed for the determination of the Sun's mean equatoreal horizontal parallax, viz.:—(1) the gravitational method; (2) the geometrical method; and (3) the physical method, and gives the principal results which have been obtained from each. He points out very fairly the chief difficulties to be encountered in applying these several methods, and directs our attention to that bugbear of the modern astronomer "systematic error." In discussing the gravitational method, perhaps Dr. Gill hardly lays stress enough on the advantages of the method which employs the secular variations of the planetary elements for determining the solar parallax—not that this method is *at present* capable of giving results equal in accuracy with those obtainable from some adaptations of the geometrical method, but as time goes on the secular variations will be determined with ever increasing accuracy, and thus, in another century or so, will furnish the means of determining the solar parallax with extraordinary exactness. The methods, both telescopic and photographic, of observing a transit of Venus are described in detail, and explanations are given of the various circumstances which militate against the deduction of a satisfactory result from this time-honoured means of determining the solar parallax. "It may be a matter of some surprise and question to future generations of astronomers why so much labour and money were expended upon so imperfect a method in 1874 and 1882." Truly, as Sir G. Airy remarked shortly after the transit of 1874, the result of our experience is to discredit Transits of Venus! Dr. Gill next discusses the manner in which oppositions of Mars or of favourably situated minor planets are used for obtaining the solar parallax. It is well known that meridian observations of Mars have persistently given a larger parallax than most astronomers would be inclined to accept. The suggestion is here put forward that this may be due to the effect of the chromatic dispersion of the atmosphere, on account of which the lower limb of the planet is coloured red and the upper limb violet; and "in the illuminated field of the telescope it is probable that the observer has a tendency to cut with his spider-web more deeply into the feeble violet limb than into the more glaring red limb." The effect of this would be to increase the value of the resulting parallax. It must be pointed out, however, that Prof. Eastman of Washington, in order to elucidate this point, has made experiments on the planet Jupiter, and has come to the conclusion that, in his case at all events, Dr. Gill's theory does not hold good. It seems more probable that the discordance in question arises from a systematic difference in the method of observing the zenith distance of a star and of a planetary disk like Mars. This brings us naturally to consider the case of the opposition of a minor planet, which in the telescope presents precisely the appearance of a star, and there would therefore be no possibility of error arising from such causes as would affect the observations of Mars. Galle's scheme for observations of Flora in 1874 at various observatories, both in the northern and southern hemispheres, was only partially successful, and must not therefore be taken as a criterion of the accuracy attainable by this method; but, as Dr. Gill points out, in any plan requiring numerous and widely spread observers, it is very difficult to secure that entire sympathy and care in details which is essential to success. Hence the superiority of a scheme of observations which can be carried out by one observer alone, such as the morning and evening equatoreal or heliometric observations of Mars, the latter of which, at all events when undertaken by such a master of the heliometer as Dr. Gill, appears to be capable of giving the most satisfactory results—the weak point of the method being, as Dr. Gill himself admits, the probability that the average light of Mars is of different refrangibility from that of the average light of the comparison stars, a circumstance which would probably give rise to systematic errors in the measures. But this source of error would be avoided by observing favourably situated minor planets in the same manner as that in which Mars was observed in 1877, and this is undoubtedly the method of determining the solar parallax to which our attention ought to be mainly directed in the immediate future. Dr. Gill gives a list of the oppositions of minor planets that will be available for this purpose till the end of the present century. In treating of the physical method Dr. Gill points out that the photometric observations of the eclipses of Jupiter's satellites, as now being carried out at Harvard College under Prof. Pickering, will probably furnish with great accuracy the data necessary for the application of this method by means of the interval in time required by light to cross the mean radius of the Earth's orbit. The theoretical difficulties which are encountered in attempting to apply the observed value of the constant of aberration in combination with the observed velocity of light to determine the solar parallax are also discussed—difficulties which will, we think, prevent this particular form of the physical method from ever becoming as satisfactory a one, at all events to astronomers, as the more strictly astronomical methods already described.

After a few words on lunar parallax, Dr. Gill goes on to speak of the parallax of the fixed stars—both absolute and relative—duly recognizing the labours of Henderson, Maclear, and Peters, but passing over, without remark in the text, the hardly less important work of Brunnow and Ball. The heliometer determinations of stellar parallax at the Cape are dwelt on, and the great accuracy of the method pointed out—an accuracy which is perhaps all that can be desired as long as astronomers are contented with determinations of *relative* parallax; but we hope the time is not far distant when they will aspire to as accurate a knowledge of the absolute parallaxes of the fixed stars.

Dr. Gill's article contains a vast amount of information on these interesting and important astronomical questions, and we leave it with a feeling of regret that it should be buried in an Encyclopædia, where it will probably be inaccessible to nine-tenths of the people who might refer to it with pleasure and profit.

A. M. W. Downing.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Partial Obscuration of the Red Spot on Jupiter.

Gentlemen,—

Prof. Young's observation of March 23 and subsequent nights, that the central portion of the red spot "is now covered by a white cloud of oval form, which is almost concentric with it, and leaves visible only a narrow ring of the red substance not exceeding 1½″ to 2″ in width," quite agrees with results obtained in this country. On many occasions during the past winter I saw that the spot was "reduced to a mere skeleton, the outline only being discernible, so that we have a very similar object to Gledhill's ellipse of 1869–70" ('Observatory,' March 1885, p. 95). I have never, though, remarked this appearance with such great distinctness as on Feb. 25, at about $12^h\ 50^m$, with a power of 252 on my 10-inch With mirror.

The observations of Mr. J. M. Offord, of St. John's Wood, with a 5¼-inch Grubb equatoreal, opportunely furnish us with good corroboration. He says ('English Mechanic,' Feb. 20, 1885, p. 539):—"The night of Feb. 11 being unusually clear and definition very good, afforded me an excellent opportunity for examining those parts of the planet immediately surrounding the red spot. My attention on commencing observations was immediately arrested by the fact that the old red spot had greatly changed, its centre being occupied by an irregularly oval white cloud nearly covering the spot, and leaving but a narrow pinkish border round its edge."

This interior obliteration of the spot has been drawing on since 1882, though it is only within the present year that it has become so very obvious. The fact that its conspicuous character has been recognized almost simultaneously by several observers does not necessarily mean that it has been a sudden phenomenon, though the appearance of the white cloud has much intensified since last autumn. Delicate markings will sometimes continue to develop during the earlier months of an opposition, and manage to elude detection until a thoroughly good night, near opposition, brings them out very strikingly, as in the present case.

At the April meeting of the R.A.S. it was mentioned that the northern equatoreal belt of Saturn had been distinctly seen with a 4$\frac{2}{10}$-inch aperture. In connection with this question, I would remark that the belt is steadily visible in a 2$\frac{1}{2}$-in. Ramsden refractor, power 94, and could be glimpsed with a slight contraction of this aperture. W. F. DENNING.

Bristol, May 1, 1885.

P.S.—From my more recent observations of Jupiter, especially on May 9, 11, and 16, when definition was unusually sharp, it appears that the red spot is becoming very much plainer. Indeed this object may now fairly be said to have come again into conspicuous prominence as an ellipse in which, though its interior contour is not perfectly even, the outline of the old red spot is exactly preserved.

When the ellipse is central, a dark narrow belt is distinctly traced on its *s.p.* side tapering to the spot on its southern boundary and actually connected with it. It is curious that nearly 18 months ago there was a belt on the *s.f.* side joined to the spot in much the same manner. A little higher, in S. latitude, there is a short dark belt very distinct and definite. In the same longitude as the centre of the red spot, and situated on the N. margin of the grea northern dark belt, there is a brilliant white spot, immediately followed by a small and compact dark marking. I have watched this white spot since Nov. 27, 1884, on which date it followed the centre of the red ellipse 29 minutes. Moving slightly faster than the latter, it was co-central with it by Feb. 25, 1885, having in the 3 months' interval gained nearly 18° of longitude, which means a displacement to the west of more than 13,000 miles. It is remarkable, however, that since Feb. 25 this feature has retained exactly the same longitude relatively to the red spot and immediately N. of its centre, where it still exists. W. F. D.

May 17, 1885.

The December Geminids, 1884.

GENTLEMEN,—

A watch of an hour's duration was taken on December 11, from $10^h\,25^m$ to $11^h\,25^m$ G.M.T. (the 10, 12, and 13 being overcast), for the purpose of recording the Geminid meteor shower.

Meteors were falling at the rate of about 17 per hour, for the [illegible] each night [illegible] years from the [illegible] [illegible] The defects [illegible]

[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Remarks.
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Rapid [illegible].
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Short and rap [illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Short and rap [illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Rapid.
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Rapid.
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Rapid. [illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Short and rap [illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Rapid. [illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Rapid.
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Quite short rapid.
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Short and rap [illegible]

[illegible]

[illegible] EDWIN F. SAWYER.
[illegible]

[illegible] of Domes.

GENTLEMEN,—

[illegible] to weaken the objections of Mr. [illegible] against the [illegible] of astronomical domes in [illegible] No. [illegible] p. [illegible]. First, the dome may be so well constructed that rain and snow can do no harm. Second, the balls may be changed at any time with very little trouble. Our dome is [illegible] feet in diameter, and is very heavy. It is mounted on six [illegible] balls of gun-metal, which roll in grooves made in iron plates, 5 in. wide and 1½ in. thick. It has been in regular use more than 9 years, and we have had little trouble from snow or rain or the shifting of the balls. The sill of the dome fits well over the bed-plate, and a projecting rim makes it very difficult for snow and rain to be forced up by the wind into the dome. Perhaps the vicissitudes of our climate make our exposure as great, in this regard, as it is anywhere in the world. Once, about five years ago, I thought it best to change the balls. This is readily done by placing a dwarf jack-screw between the plates and giving a few turns to the screw. One man can change the balls as he may choose in a few minutes. It seems to me that the greatest trouble in the use of balls is the gradual accumulation of dust in the grooves, and the liability to lose their sphericity by gradual wear. Still a good set of balls will probably last as long as wheels or rollers, and can be replaced at less cost and with greater facility. Besides, if the balls are turned frequently in the grooves, their gradual wear will leave them still nearly spherical.

Morrison Observatory, Glasgow, Mo., C. W. PRITCHETT.
April 21, 1885.

The Comets of 1618.

GENTLEMEN,—

As the corrected proof of my letter to you of the 7th inst. appears to have miscarried, I would crave space for a few lines with regard to it.

The end of the first paragraph therein is likely to create a wrong impression, for the meteoric observation (if it were such) referred to is that of Schickard on the 20th of *October*. What I wished to dwell upon was that if, as Newton conjectured, the observation made by Benjamin Ursin on the 30th of that month were really of the great comet discovered on November 30, seen before its perihelion passage (which took place on November 8th), and not, as Pingré supposes, of a meteor, Schickard's may possibly have been of the same object, so that neither of them would be meteors.

I must also point out a misprint in the ninth line of the second paragraph, where the word "ante" should be "puto." The word "Borrhapeliotes" is evidently adopted from the Greek *βορραπηλιώτης*, which is only to be found, I believe, in the Paraphrase of Ptolemy by Proclus. But as *ἀπηλιώτης* is an ordinary Greek word for east or east wind, it is evident that Pingré is right in translating "ad Borrhapelioten" into "au nord-est."

Kepler's idea that the second and third comets of 1618 were one and the same which had divided itself into two, is sufficiently disproved by Pingré, who appropriately applies to it the expression "Quandoque bonus dormitat Homerus."

The old English historian, Arthur Wilson, in his account of the reign of James I., refers to the last comet of 1618, though he gives the duration of its appearance incorrectly. He tells us that the common people regarded the comet as a flambeau for the funeral of Queen Anne (commonly called Anne of Denmark), who however, did not die until the 2nd of March 1619; and it is amusing to read the evident claim he makes to superior discernment in considering that it had more to do with the war then breaking out in Bohemia, which was the beginning of the thirty years' war in Germany.

Yours faithfully,

Blackheath, 1885, Mar. 30. W. T. LYNN.

P.S. (April 8).—Since I wrote the above, I have received an obliging communication from Mr. Dreyer at Armagh, in which he calls my attention to the fact that Lubienitzky gives an account, in his 'Theatrum Cometicum,' of the object said to have been obsreved by Ursinus at Cologne. One thing is clear that the date October 30 is to be taken as new style; but although Lubienitzky speaks of other observations made early in November in Spain and France, it is difficult to feel sure that they, as well as that of Ursinus on October 30, do not refer to meteoric appearances. Bessel gives the positions of the last comet of 1618, for several dates before its perihelion passage, in the 'Berliner Jahrbuch' for 1808, p. 122: and, as Mr. Dreyer remarks, they seem to be consistent with the theory that it was observed by Ursinus. I hope that my last letter to you may produce a quotation of his original observation from the treatise to which I referred.—W. T. L.

NOTES.

Photographing the Corona.—During the partial solar eclipse of March 16, Mr. W. H. Pickering endeavoured to repeat Dr. Huggins's experiments on photographing the corona, but, unfortunately, without success; for though very corona-like effects were produced, as no two pictures were alike, and as the corona was as well marked in front of the Moon as that on the other side of the Sun, it was clear that they were not due to the real solar corona. The failure of his attempts evidently shook Mr. Pickering's faith in the genuine character of the coronal images on Dr. Huggins's photographs, and he expressed his doubts not obscurely in a letter appearing in the number of 'Science' for April 3, 1885. The account, however, which he there gave of his experiments was sufficient to show all who have attentively followed Dr. Huggins's published descriptions of his investigations, that Mr. Pickering had failed to observe precautions which Dr. Huggins had laid down as essential; and the latter has since pointed this out with his accustomed clearness of expression. As Mr. Pickering's letter to 'Science' has been reprinted in several English publications, it may be well to reproduce two or three of the principal points of Dr. Huggins's reply.

Mr. Pickering's first error lay in using an object-glass to produce his image. This, Dr. Huggins had found from his earliest experiments, would give just such fictitious coronal appearances as Mr. Pickering had obtained, and he therefore soon discarded the lens for a mirror. Then Mr. Pickering, instead of placing his drop-slit in the focal plane of the telescope, had attached it to the lens, where it would produce marked diffraction effects. Further, Dr. Huggins had been careful to test his own apparatus, and to secure that when the sky is free from cloud, but whity from the strong scattering of the Sun's light, his plates should show the Sun "well defined upon a sensibly uniform surrounding of air-glare, but without any indication of the corona. It is only when the sky becomes clear and blue in colour that coronal appearances present themselves with more or less distinctness." It is clear that Mr. Pickering had not subjected his instrument to any such test; and it is manifest that Dr. Huggins is correct in insisting that "any apparatus intended for photographing the corona must fulfil perfectly these conditions before any serious attempts are made to obtain the corona."

Dr. Huggins remarks on several other points in Mr. Pickering's letter which seem to indicate errors of exposure and of photographic manipulation; but the above are of themselves sufficient to show that Mr. Pickering did not observe those conditions to which Dr. Huggins had previously called attention "as essential in a matter of such extreme delicacy as photographing the corona," and that his failure therefore does not convey the slightest presumption against the true coronal character of Dr. Huggins's photographs; indeed, it brings into greater prominence the caution and skill with which Dr. Huggins has proceeded.

STELLAR PHOTOGRAPHY AT PARIS.—The new instrument for stellar photography has just been mounted at the Paris Observatory; and though the final adjustments have not yet been made, the success of the first experiment with it seems to have been most complete. The instrument consists of two object-glasses placed side by side, the optical axes being parallel; the one (aperture 9·5 in. and focal length 11 ft. 10 in.) is for eye-observation and acts as finder; the other (aperture 13·4 in. and focal length 11 ft. 3 in.) is achromatized for the chemical rays, and is used for photography. This latter object-glass was constructed by the Brothers Henry; it covers very distinctly a field of 3° in diameter without the employment of any diaphragm. With this apparatus, which is mounted as in the English equatoreal form, an exposure of an hour gave a fine negative which, on a surface 10 inches square, representing 5 square degrees of the heavens, showed 2790 stars, from the 5th to the 14th magnitude; whilst traces of stars of the 15th magnitude could be detected. Stars of the 14th magnitude had a diameter of about $\frac{1}{1000}$ inch. To have constructed such a chart by the ordinary astronomical methods would have entailed many months of hard work.

Admiral Mouchez, in presenting this first photograph to the Academy of Sciences, remarked on the advantages which a complete photographic survey of the heavens would secure. The elder Herschel had made a number of gauges of different parts of the sky; this would complete the work he began, but in a more satisfactory manner, and would present us with a record not only of the numbers and general distribution of the stars, but also of their exact positions and magnitudes. Mr. Gill has offered to co-operate with the Paris Observatory in this survey; and Admiral Mouchez estimated that six or eight years would suffice for the completion of this work, which would give us a record of the heavens absolutely unattainable in any other way.

TABLES METEOROLOGICAL AND PHYSICAL. BY ARNOLD GUYOT.—A new (the fourth) edition of this well-known work has been recently published. For those of our readers who may be yet unacquainted with the contents of this bulky volume of above 700 pages (which forms one of the Smithsonian Institution Series, and which in this issue has received some additions), it will possibly be best to indicate briefly what may be found therein. It contains extensive tables for the conversion of thermometrical values in any one of the recognized scales into values in each of the other scales, a large collection of hygrometrical tables, tables for converting barometer readings in one scale into equivalent values in other scales, also for rain-records, and tables for reduction of barometer readings to the freezing-point. There is also a very considerable collection of hypsometrical tables, thermometric as well as barometric, and tables for conversion of geographical measures of distance and surface according to the units adopted by different nations. A section of miscellaneous tables, geodetic and geographical, includes also one giving the length of time of insolation for any latitude and for any day of the year. The tables for correcting series of thermometrical observations for periodic and non-periodic variations, the former giving the hourly deviations of temperature from the mean in each month for a large number of different places throughout the world, and the latter for a smaller number of places, the deviation of the temperature of each individual month through series of years, from the average temperature of the month have not been changed. This is, perhaps, a little unfortunate, considering that since the publication of the last edition of the work, some twenty-five years ago, much material has become available for improvement of the tables of periodic variations, as well as for extension of the tables of non-periodic variations, although the giving of information of this kind was not the primary object of the work, whose value consists in the extensive series of fundamental tables which it contains. The respected author, who for many years occupied the position of Professor of Geology and Physical Geography in Princeton College, unfortunately did not live quite to finish this new edition of his work, which has been brought to a completion by his assistant, Professor William Libbey. W. E.

THE MADRAS OBSERVATORY.—We learn from the Administration Report upon this Observatory, for the year 1883, that the number of observations made with the Meridian Circle during the year was 2453, bringing up the total number on hand, awaiting publication, to 50,878. Only a few hundred more are required to finish the Catalogue of over 5000 stars, and no fresh work of the kind will be commenced until that already accomplished is published. The arrangement of the separate annual catalogues is fairly in hand, but these will, of course, require reducing up to the adopted epoch 1875 before the final Catalogue can be formed. Seven or eight volumes of separate results will therefore have to precede the *publication* of the final work. We venture to think that this postponement of the publication of the Catalogue is a mistake. Astronomers have long been looking for some published results of the meridian observations made at Madras, and the speedy publication of a Catalogue of 5000 stars would do much towards restoring the Madras Observatory to the position, as a scientific institution, which it formerly held. A few observations of minor planets were made with the equatoreals during the year. The variable star atlas and comparisons of these objects were in abeyance throughout 1883. Mr. Pogson states the transit-circle, the 8-inch equatoreal by Troughton and Simms, and the 6-inch equatoreal by Lerebours and Secretan were all in good working order, but complains somewhat bitterly of the inadequacy of the instrumental means at his command for undertaking modern astronomical researches.

THE ALLEGED DISCOVERY OF TEMPEL'S COMET.—A correspondent has written to us taking us severely to task for the insertion of the following sentence in our notice of the Meeting of the Liverpool Astronomical Society on April 13:—"Mr. R. Wilding, Hon. Curator of Preston Observatory, announced his discovery of Tempel's Comet. Its position on April 8 was R.A. 12^h and about 19° N. Dec. The nucleus appeared like a 13th mag. star surrounded by a nebulous disk." There can be very little doubt but that the object observed was a nebula, as our correspondent suggests; but we would point out that we are in no way responsible for the accuracy of statements made by the members of the two astronomical societies at their respective meetings. We *are* responsible that our report should reproduce, as closely as our space permits, the gist of what passes at those meetings, but the speakers or writers of papers must themselves alone be held responsible for the statements they make.

A RELIC OF SIR W. HERSCHEL.—Dr. R. J. Ryle has presented a 6-in. mirror figured by Sir W. Herschel, and which had belonged to Mr. Dawes, to the Royal Astronomical Society.

MINOR PLANETS.—The Minor Planet No. 246 has received the name of Asporina. The name Eukrate, given to No. 247, was given by Prof. E. Schoenfeld of Bonn. Dr. R. Luther points out that it is necessary that observations with powerful instruments should be secured of Eukrate during the present month in order to improve our knowledge of its orbit. It will be far south in 1886 and 1887.

THE DENSITY OF THE EARTH.—Herrn König and Richorz propose the following new method of measuring the density of the Earth. On the horizontal surface of a large block of lead in the shape of a parallelopiped is placed a balance whose scales swing very near the surface. A vertical hole is bored through the block, directly under the point of suspension of each scale; a second pair of scale-pans is suspended below the block by wires attached to the upper scale-pans and passing through these openings. The balance thus consists of two pairs of scale-pans having the lead between them. One mass is placed in the upper and the other in the opposite lower pan of the scales. The attraction of the block will make the lower one lighter, the upper one heavier. The weight in the lower pan is then removed to the pan immediately above it, and *vice versâ*. The attraction of the block makes heavier the weight which before it made lighter, and *vice versâ*, thus causing a difference in the weights amounting to four times the attraction of the block.

NEW NEBULÆ.—M. Stephan publishes in the 'Comptes Rendus,' No. 17, a list of 100 nebulæ discovered and observed at the Marseilles Observatory. Besides a very complete description of each, there is given the exact R.A.'s and declinations for epoch 1885·0. The list is also published in No. 2661 of the 'Astronomische Nachrichten.'

TWO NEW VARIABLES.—E. Schönfeld has found two variables in Libra. The two stars are close together and very small—

R.A. $14^{h}\ 32^{m}\ 18^{s}{\cdot}0$ $\quad -17^{\circ}\ 1'{\cdot}8$
R.A. $14\ \ 32\ \ 31{\cdot}9$ $\quad -17\ \ 1{\cdot}5$

WE greatly regret to have to record the death, on May 19, of the Rev. T. W. Webb, M.A., in the 79th year of his age. Mr. Webb was a Prebendary of Hereford Cathedral, and Vicar of Hardwick, Herefordshire. He frequently wrote, chiefly on astronomical subjects, in 'Nature,' the 'Astronomical Register,' the 'Intellectual Observer,' the 'Student,' &c., but his principal contribution to astronomical literature was the widely known and highly valued 'Celestial Objects for Common Telescopes,' a work by which he will be long and honourably remembered.

Maxima and Minima of Variable Stars in June 1885.

M, signifies maximum ; *m*, minimum.

June	1	R Coronæ, M.	June	16	U Monocerotis, M.
	2·4	W Virginis, *m*.		17	W Herculis, *m*.
	3	R Vulpeculæ, M.		17	R Crateris, M.
	3	Z Virginis, M.		19·4	T Monocerotis, M.
	7	χ Cygni, *m*.		19·7	W Virginis, *m*.
	8	R Pegasi, M.		21	U Virginis, *m*.
	10	R Geminorum, M.		21	R Tauri, M.
	10·6	W Virginis, M.		22	R Virginis, M.
	11	R Aquilæ, *m*.		22	S Leonis, M.
	11·4	T Monocerotis, *m*.		23	R Sagittæ, *m*.
	12	U Cygni, M.		25	R Lyræ, *m*.
	14	S Vulpeculæ, *m*.		26	T Sagittarii, M.
	14	R Leonis, *m*.		26·7	β Lyræ, *m*.
	15	R Ceti, M.		27	T Ursæ Majoris, M.

Variables of Short Period.

δ Cephei.	η Aquilæ.	U Cephei, *m.*	δ Libræ, *m.*
d.	d.	h m	h m
June 5·45, *m.*	June 10·4, M.	June 9 15 16	June 7 11 22
12·45, M.	17·6, M.	14 14 56	14 10 56
21·55, *m.*	22·4, *m.*	19 14 35	21 10 30
28·55, M.	24·7, M.	24 14 15	28 10 4
	29·5, *m.*	29 13 54	

U Sagittarii.	W Sagittarii.	S Cancri, *m.*	U Coronæ, *m.*
d.	d.	h m	h m
June 12·6, *m.*	June 10·5, *m.*	June 14 11 55	June 5 14 44
15·6, M.	13·6, M.		12 12 27
19·4, *m.*			19 10 9
22·3, M.			

Astronomical Memoranda, June 1885.

Equation of Time:—Sun *before* Clock, June 1, $2^{\mathrm{m}}\ 25^{\mathrm{s}}$; June 11, $0^{\mathrm{m}}\ 34^{\mathrm{s}}$; June 21, *after* $1^{\mathrm{m}}\ 30^{\mathrm{s}}$; June 30, $3^{\mathrm{m}}\ 23^{\mathrm{s}}$.

Sidereal Time at Mean Noon:—June 1, $4^{\mathrm{h}}\ 40^{\mathrm{m}}\ 32^{\mathrm{s}}$; June 11, $5^{\mathrm{h}}\ 19^{\mathrm{m}}\ 58^{\mathrm{s}}$; June 21, $5^{\mathrm{h}}\ 59^{\mathrm{m}}\ 23^{\mathrm{s}}$; June 30, $6^{\mathrm{h}}\ 34^{\mathrm{m}}\ 52^{\mathrm{s}}$.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
June 0	15 51	8 3	344 23	−0 29	158 35
5	15 48	8 8	346 18	+0 8	92 24
10	15 45	8 12	348 21	0 43	26 13
15	15 45	8 15	350 29	1 19	320 3
20	15 45	8 17	352 41	1 54	253 51
25	15 47	8 18	354 55	2 29	187 40
30	15 49	8 18	357 12	+3 2	121 30

The Moon.

rises.	*rises.*	*sets.*	*sets.*
h m	h m	h m	h m
June 1 10 37	June 9 14 21	June 17 11 20	June 25 15 21
2 11 10	10 14 58	18 11 47	26 16 6
3 11 39	11 15 43	19 12 13	27 *rises*
4 12 6	12 *sets*	20 12 39	28 8 39
5 12 31	13 8 32	21 13 5	29 9 14
6 12 55	14 9 26	22 13 33	30 9 44
7 13 21	15 10 11	23 14 5	
8 13 49	16 10 48	24 14 41	

June $5^{\mathrm{d}}\ 12^{\mathrm{h}}\ 5^{\mathrm{m}}$ Last Quarter. June $12^{\mathrm{d}}\ 10^{\mathrm{h}}\ 42^{\mathrm{m}}$ New Moon.
19 1 48 First Quarter. 26 23 18 Full Moon

[illegible]

[illegible] Ec. R.
[illegible] Ec. D.

[illegible]

Day.	Star.	Mag.	Disap.		Angle	Reap.		Angle.
			h	m	°	h	m	°
[illegible]	[illegible] Capricorni	[illegible]	[illegible]	[illegible]	[illegible]	13	7	248
[illegible]	[illegible] Capricorni	[illegible]	[illegible]	[illegible]	[illegible]	14	22	229

The R.A. and N.P.D. of Planets and the Solar Co-ordinates are given for Greenwich Mean Noon: in all cases Greenwich Mean Time is used, reckoning from noon to noon. as in previous years the angles for Occultations are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

ERRATA.

No. 97, page 188, line 30, for 2''·4479 *read* −2''·4479.
No. 98, page 195, line 39, for ε and δ *read* ε and δ Serpentis.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 99. JULY. 1885.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, June 12, 1885.

E. Dunkin, F.R.S., *President*, in the Chair.

Secretaries: E. B. Knobel and
Lieut.-Col. G. L. Tupman.

The Minutes of the last Meeting were read and confirmed.

Col. Tupman announced that 62 presents had been received since the last Meeting; amongst these was a photograph of part of the Milky Way, taken by the Brothers Henry of Paris, with the new object-glass of 13 inches aperture and 11½ feet focal length, which they had themselves constructed for this purpose, and which was mounted side by side on the same axis with an ordinary refractor. The plate was exposed for 60 minutes, and the present photograph had been enlarged twice from the original.

The President. The photograph shows stars down to the 14th magnitude.

Mr. Knobel read a paper by *Mr. W. F. Denning*, on the Velocities of Meteors. The author referred to the discussions at the Society in December and May, as to the velocities of meteors and the speed necessary to explain the occurrence of fixed and long-enduring radiant-points. He wished to say that no such velocities had been observed, and it was impossible that the fixed radiant-points could be explained by that means. He agreed with Colonel Tupman as to the difficulty of accounting for these long-enduring showers, and that the velocities indicated by Mr. Proctor had never been recognized. The meteors falling from stationary radiant-points, as described in the paper read in December, appeared to be of an ordinary character, for their motions did not differ essentially from the parabolic velocities exhibited by planetary meteors. Mr. Ranyard had asked whether the speed from any of the assumed radiants had been determined. In several observations of large meteors the velocity had been computed, though it was [illegible] estimating the [illegible] duration of meteor showers [illegible] the improbability that the velocity [illegible] suggested by the [illegible] explanation of the long-enduring showers. The paper [illegible] to show that the [illegible] with the increase of the time in which [illegible] the Earth [illegible] that the [illegible] which [illegible] than that of the ordinary stream.

[illegible] Denning [illegible] to show that the velocity [illegible] great [illegible] which has been [illegible] different phenomena [illegible] as great as ordinary meteors would make them appear [illegible] they would be lost sight of the [illegible] were seen.

Colonel Tupman: I do not think the Fellows of the Society generally, Mr. Proctor [illegible] on this subject. I will endeavour t explain. [illegible] suppose the limit of the Earth's atmosphere to b about 100 miles above us. Thousands of shooting-stars have bee measured, at that height, or rather at a height of 80 or 60 mile from radiants in all parts of the sky. The height at which the begin to appear may be said to be constant for all of them. Th relative velocity of meteors which meet the Earth is greater tha the average, and they begin to appear a little higher, as, for instanc the November meteors; whilst for meteors which overtake th Earth, the relative velocity may be reduced to 7 or 8 miles a secon and they do not appear so high; but there is little difference, as t the height at which they appear, between the extremes of the meteo which have been measured. The only way in which Mr. Procto assumption of increased velocities could possibly exist would be the meteors encountered the atmosphere at heights many tim greater than 80 or 60 miles—in such proportion, in fact, as to mai tain the average apparent angular velocity. These long-enduri radiants observed by Mr. Denning are not by any means accident radiants, or of a different category to those radiants the heights whose meteors have been accurately investigated, but in many cas are the very same. We must conclude that these long-radia meteors are at no greater elevation than the others, and therefo that their velocities are no greater than velocities observed in other kinds of meteors—that is, what is called parabolic. Colon Tupman proceeded to explain that the velocity of meteors wh they came in contact with the Earth's atmosphere could in near every case be represented by the expression $v\sqrt{2}$, where v is t orbital velocity of the Earth, such velocity being that of a bo moving about the Sun in a parabolic orbit.

Mr. Bryant read a paper on the Elliptic Elements of Com Ross. He said the paper was chiefly mathematical. His attentio had been drawn to the fact that such orbits as he had seen computed of this comet did not present such agreement as might be expected on the assumption of parabolic motion, and therefore he undertook to investigate the elliptic elements. The observations of the comet had been few in number. Acting on the advice of Dr. Hind, he rejected all the observations except those of Mr. Tebbutt; but he failed to obtain an agreement with a parabola; and therefore he tried elliptical elements and obtained his results by differential equations. He had derived the two sets of elements given in the paper, and both the orbits satisfied the observations with a maximum error of $0^{s}\cdot06$ in R.A. One gave a period of 87 years and the other 94 years, a difference of 7 or 8 per cent. on the total amount; nevertheless, both orbits satisfied the observations.

Mr. Knobel read a paper on "Al-Sūfi's Star-magnitudes." He said that in the 14th vol. of the Annals of Harvard College Observatory, Prof. Pickering had called attention to some alleged differences in the star-magnitudes of Al-Sūfi and Ulugh Beg, which might lead one to infer that Ulugh Beg had made some original observations on the brightness of stars. In the 'Monthly Notices' for 1879, Mr. Knobel had given a complete translation of the magnitudes in the R.A.S. Persian MS. of Ulugh Beg, a reference to which would have disposed of a large proportion of the alleged differences.

To investigate the point he had translated the magnitudes of Al-Sūfi as given in two Arabic MSS. in the British Museum and the India Office Libraries, and appended to the paper the different readings he had found, compared with Prof. Schjellerup's translation of MSS. at St. Petersburg and Copenhagen. The result arrived at was to show that it is extremely doubtful whether any difference exists between Ulugh Bey and Al-Sūfi, except what is due to errors of transcriber or translator.

The President. We are very much indebted to Mr. Knobel for the way in which he goes into these historical subjects. I think no other person now amongst us would have the patience to investigate such ancient MSS. so thoroughly, and I am sure you will join with me in returning thanks to Mr. Knobel for his interesting paper.

Mr. Knobel read a paper by *Professor Pritchard* on the criticisms of the Astronomer of Harvard College on the Oxford Stellar Photometry. Professor Pritchard referred to a statement made by Professor Pickering that the results derived from the wedge Photometer should theoretically be affected by the brightness of the background, and that such an effect actually appeared to occur. This statement surprised Professor Pritchard, because in the application of the wedge he had taken the most scrupulous care lest any such systematic error should lurk in it, especially in regard to any varying of the background against which the star should be projected. He had especially felt it necessary to be practically assured that even bright moonlight should produce no [illegible] the determination of the relative magnitude. [illegible] that this was the case in the observations [illegible] During the three years in which [illegible] the star Polaris had been extinguished [illegible] and he explained that he had divided [illegible] corresponding to the four phases [illegible] the phase. The result of the extinction [illegible] as shown by a table given in the paper, [illegible] and [illegible]. A variation of [illegible] reading implied a variation of $\frac{1}{50}$ in [illegible] readings for the four quarters of the [illegible]. The paper then proceeded [illegible] brightness of the field arising [illegible] and showed that there was no suffi- [illegible] errors in the Oxford photo- [illegible] in examining the Oxford [illegible] grounds for denying the existence of [illegible] Professor Pickering. [illegible] on behalf of the wedge [illegible] either in relation [illegible] or the brightness of [illegible] was important, however, to see what [illegible] of the investigations made [illegible]. The accuracy of the magnitudes deter- [illegible] separate determination [illegible] shown from the Harvar [illegible] not be necessarily concede [illegible] limited to three nights. Thes [illegible] with the Oxford work [illegible] be published by the Uni- [illegible] comparison one thing woul [illegible] photometry to the deter- [illegible] have made an enormous [illegible] wedge the difference in [illegible] might be ascertained within the limit [illegible] magnitudes; but he hoped that [illegible] inconsistent with the [illegible] and labour which charac- [illegible] Harvard College Observatory work.

[illegible] As far as I can understand Professor Pickering's work, the [illegible] has been this—that from his experience he thought that determinations of the magnitude o a star on three separate nights would be sufficient to fix the magnitude within the degree of accuracy he hoped for. But as, i the case of cloud, the measurement might be disturbed he ha done what appears to be perfectly reasonable and right—he ha multiplied the observations and taken the mean. In a work o this kind a certain number of discrepancies must be expected, an Prof. Pickering has shown by multiplying his observations that h did know that these discrepancies would occur. It therefore does not, in my opinion, give anything like a fair idea of the accuracy of Prof. Pickering's work to pick out a number of cases where the number of observations has been enlarged; and as far as I have been able to examine the work, it is one of extreme accuracy for a work of that kind. With regard to the point that Prof. Pritchard has not been able to detect the differences of magnitude of the stars, whether there be bright moonlight, or no moonlight at all, well, all I can say is, that if the observations are purely differential, and if the stars have been compared when the illumination of moonlight has affected them to the same extent, then, undoubtedly, differences would not appear—whether made with moonlight or without moonlight, they may agree very well; but if it be maintained that the observations of magnitude do not vary to any sensible extent in proportion to the brightness of the field within which they are made—well, it may be true [Laughter]; but I must confess that it appears to me a proof that the photometer is not accurate enough to discriminate under the circumstances. When this method was first brought forward, it was supposed that by means of the wedge-photometer you could get more accurate observations than by limiting apertures. Subsequently the method of limiting apertures has been applied to determine the constants of the wedges, and the observations have been made more and more differential. If the observations are uniformly differential, an amount of accuracy can be attained in observations of this kind, such as is quite out of the question when dealing with absolute quantities. If, therefore, the results appear to prove that the magnitudes of stars remain the same under different atmospheric illumination, and that in case of bright moonlight they are not affected by that moonlight, then I should be afraid that it would prove that the photometer was not so accurate an instrument as we have had reason to suppose.

The Astronomer Royal. I have had the advantage of examining Professor Pritchard's work, and I must say I think the Society ought to congratulate him upon the accuracy he has introduced into celestial photometry by his wedge-photometer. I was somewhat prejudiced against this instrument, so I think my testimony in favour of it may be accepted with all the more reason. I had always thought that the point at which a star would be apparently extinguished would depend very much upon the sensitiveness of the eye at the moment. I found this was not borne out in the least by the observations. You have seen by the results given to-night that the point of extinction, which is an absolute measure and not a relative measure, varies merely by the amount of $\frac{3}{50}$ths of a magnitude for Polaris under the different circumstances of bright moonlight and of no moonlight. I should be inclined to say no photometric results yet given are so accurate. I naturally feel a suspicion when results are too accordant, and should be disposed to think that the measures have been always repeated under the same conditions and are all subject to similar errors. I have looked into Prof. Pritchard's results, and examined the measures as far as I could, and I must say, so far as I have gone, I have satisfied myself that there is nothing at all suspicious about them. This method of extinction seems to me a valuable method in photometry. It seems to result that the sensitiveness of the eye is practically a constant, so that instead of having to compare one quantity with another, as in equalizing the lights of two stars and thus combining the errors of two measurements, we have only one quantity to measure, and hence only one error of measurement t deal with. That is an important point. There is another questio raised by Prof. Pickering, which Prof. Pritchard has met b bringing forward numerical results, namely, that of the influenc of moonlight; but besides the mere numerical result there is common-sense point of view of the question. We have a sta seen on a bright background of moonlight, and we apply th wedge-photometer, which, by extinguishing the light, makes th star disappear. It is admitted that the star's light is enormousl brighter than the background. It stands to reason that long before the star has disappeared by diminution of the light, the background will have faded out of view, and will have no effect whatever on the retina. Therefore I think this objection falls utterly to the ground. I have examined the theory of Prof. Pickering's photometer, and I can conceive it possible that, as Prof. Pritchard has suggested, there might be some residual effect from the background if two stars of different brightness are compared, because there is scattered light from the one star which may be brighter than that from the other, interfering to a certain extent with the light of the fainter star.

It seems to me that the wedge-photometer is a very simple instrument in which there are very few sources of error; and the only source of error that I see is that the sensitiveness of the eye may vary from time to time and from instant to instant. The simple test of that is, whether the same reading is obtained night after night for a star of constant brightness, like Polaris, or a different reading. Prof. Pritchard has given the actual readings at which Polaris disappeared, and if these agree within a small fraction of a magnitude it is to be presumed that his measures give not only the relative magnitude, but that they give practically absolute measures of brightness.

Mr. Stone. Are all the observations given or only a selection?

The Astronomer Royal. I do not know whether Prof. Pritchard has published them all, but I looked into them myself. I am not competent to stand forward and propound Prof. Pritchard's methods. I can only give them second-hand. I was very much interested in the question, and inclined to doubt his results. I felt they were almost too good to be true. My own experience of measuring star-magnitudes did not lead me to believe they could be measured so accurately. Prof. Pritchard showed me the results obtained with two different telescopes and by different observers, and with different apertures, and he also showed me the different individual readings. I cannot say whether they are printed; but I had the opportunity of examining them just as they were, and I could form a general idea of their accordance and was very much impressed by that; but I can only give the meeting the impression I derived from the examination.

Mr. Common. I am extremely surprised that the extreme range of difference of magnitude is so small as $\frac{3}{50}$ths.

The Astronomer Royal. That is the mean of a large number of observations.

Mr. Common. What is the extreme range?

The Astronomer Royal. If you will excuse me interrupting you, I may explain that when I spoke of $\frac{3}{50}$ths of a magnitude it was in reference to the possible systematic effect of moonlight. In each case there were about 1000 observations. That is a question of the effect of moonlight, and is quite a different matter. I do not mean to say, if three observations were to be taken, that we should expect any three to agree within $\frac{3}{50}$ths of a magnitude exactly. There are two different questions involved: one is whether there are systematic errors depending upon moonlight, and the other is whether there are large accidental errors of observation. Prof. Pritchard's point is that in his observations there is no appreciable systematic error depending on moonlight, which he seeks to prove by bringing forward his observations of Polaris, grouped according to the quarters of the Moon in which they were made, and the other is as to whether there is a large accidental error either in his observations or in Prof. Pickering's; and he implies, I think, that there is a much larger error in Prof. Pickering's observations, which involve the measure of two quantities, than in his, which only involve the measure of one quantity, viz. extinction of the star observed.

Mr. Common. Prof. Pritchard, in his paper, as I understand, gives certain points of extinction, and the number that represents either the extent of the wedge in or out that would correspond with a certain extinction, which, I suppose, is the average of a number of observations. In one quarter of the Moon's age he has over 1000 observations, which correspond to 4·7 of the wedge. That is an average, I presume.

The Astronomer Royal. Yes.

Mr. Common. He does not state what is the extreme range of difference here, because this is an absolute measure; and surely if it is an absolute measure we all know there must be nights when you find, according to the telescope you use, there are one or two magnitudes more or less visible. There must be a range to this. It appears to me that Prof. Pritchard here gives the average of his readings in this absolute measure, and he gives the extremes of Prof. Pickering, and not the average.

The Astronomer Royal. They are two different questions. Prof. Pritchard does not compare them. I think that is quite a misconception. He does not give them as contrasting with Prof. Pickering's ranges.

Mr. Common. But he gives the ranges.

[illegible] I wish [illegible] the question [illegible] Prof. Pickering has raised [illegible] Prof. Pritchard's results, that they are affected by [illegible] of the Moon. In order to show that there is no appre[illegible] moonlight, Prof. Pritchard discusses his [illegible] arranged according to the first quarter, [illegible] third quarter, and fourth quarter of the Moon [illegible] effects of moonlight. That is merely [illegible] whether there is an effect resulting from moonlight or not [illegible]. Another question is to explain [illegible] Prof. Pritchard's results and Prof. Pickering's results [illegible] the individual observations made [illegible] are subject to considerable accidental [illegible] systematic error, which might [illegible] Prof. Pritchard's results. The other is the accidental [illegible] Prof. Pritchard conceives to affect Prof. Pickering's results [illegible] to make them less trustworthy when the number [illegible] is reduced to three. Prof. Pritchard [illegible] Prof. Pickering had made a large number [illegible] would get an accurate result, but he [illegible] with a limited number of extinctions [illegible] than Prof. Pickering's; and so far [illegible] to agree with him.

[illegible] given a certain point of [illegible] effect at all if it does not come [illegible] the star observed. If you extinguish the star, it would have no [illegible] but I may mention [illegible] Prof. Pickering's photometer or [illegible] we shall ever have to be [illegible] (Laughter.) There is no [illegible] that I have seen lately. In [illegible] photographs with a Ross's lens over a [illegible] and I put them away as experiments [illegible] but lately I have brought them forward [illegible] comparing them with photographs [illegible] I believe, Mr. Woods. [illegible] in certain stars is [illegible] and from the investigation of many photographs of the [illegible] nebula of Orion I believe that every star in the heavens is more or less subject to variation in light, and that much of the [illegible] in these photometric researches is due not so much to [illegible] errors, as to the difference in light the star gives; and [illegible] the only way we can get to any absolute determination [illegible] of stars in one respect, that is in chemical power, is [illegible] photography, and I think that will be the photometer of the [illegible]

[illegible] With regard to this method of extinction, I had, [illegible] Greenwich, some experience of it. I had a small photometer of my own there consisting of two tourmalines, and by turning one relatively to the other I could get an angle of extinction. I found by that angle of extinction I could make very good differential observations indeed. Mr. James Carpenter also used it, and I found his differential results agreed exceedingly well with mine, but the angle of extinction which Mr. Carpenter gave differed entirely from the angle of extinction which I gave.

The Astronomer Royal. The point is—was it constant for yourself on different nights, and also for Mr. Carpenter?

Mr. Stone. Using this instrument for taking the differences between the magnitudes of certain stars, Mr. Carpenter's observations and my own agreed; but using it as an absolute measurer, they disagreed as to the angle at which the stars disappeared. I am perfectly certain the angle at which the stars disappeared was entirely different according to whether the sky was clouded or clear.

Mr. Knobel. I have looked very closely into Prof. Pritchard's observations, and have had a great deal of correspondence with him, in criticising and questioning the results. I think there has been one point not brought clearly before the Society, and that is, the method adopted in making the observations. I have made many hundreds of observations of extinctions of stars on the method of limiting apertures, and I do not apprehend there is any fundamental difference between the method of extinguishing a star by limiting apertures or by that of employing a wedge. But I did find this one certain result, that is, that the operation has to be performed rapidly. The star disappears at a larger or smaller aperture according as we move the apparatus quicker or slower, and that is perfectly explicable. I have questioned Prof. Pritchard on the method he adopts in making these observations, and I believe that point was never explained to the Society. The operation is performed by sliding the wedge in the groove in which it is fixed in the apparatus until the star is brought down to a low brightness, and then the screw is rapidly turned, and the whole gist of it is, that it is rapidly turned and extinguished. Now by means of a limiting-aperture contrivance, in which I rapidly extinguish the star, I can make the observations consistent; but if I take a little more time over it the observations are inconsistent, because the pupil of the eye dilates. Here, then, is the possibility of systematic errors coming in. We all know that in turning a micrometer screw how easy it is to unconsciously turn it exactly as much at one time as at another, and I mentioned this point to Prof. Pritchard as to the possibility of systematic error creeping in by the wedge being systematically pushed into the same point by the same amount of the twist of the screw extinguishing the star in the same part of the wedge in consecutive observations, and I cannot get over the point that there may be systematic errors in his method of observation. As regards the illumination of the field, I think that might be elucidated if Prof. Pritchard were to extinguish and determine the relative brightness of much fainter stars than he has attempted, because in brighter stars the background is extinguished before the star, and in fainter stars the stars are extinguished before the background becomes black. Therefore the conditions are reversed, and there would not be a consistent light-ratio between the brightest stars and the very faintest stars.

Lord McLaren. I may mention as confirmatory of what Mr. Knobel has stated that having made some experiments with the wedge photometer, there were half a dozen nights during which I used it I found that when the wedge was moved with great slowness across the field that I was able to follow stars even of the third magnitude to the extreme end of the wedge; they were still visible as very faint points of light; while when it was moved rapidly they were extinguished long before they reached the extreme edge. It is evident that this is a point which must still be investigated to see whether it is not possible, by very carefully watching, that you may find there is considerable latitude in the scale before the point of extinction, or that you cannot get the exact point of extinction within such narrow limits as has been supposed.

The Rev. Father Perry. Would you kindly tell the meeting why Prof. Pritchard moves it so rapidly when he has brought the star to a small magnitude?

Mr. Knobel. To extinguish it. (Laughter.)

The Rev. Father Perry. To make the observations accord, I suppose.

General Tennant. Mr. Common and I spent part of one morning with Prof. Pritchard, and my recollection is, the point he observes is not that of extinction but of minimum visibility; and I look upon that as a point which can be very much more accurately observed. But there is another matter, as to the difference between Prof. Pickering's comparatively wild results and Prof. Pritchard's. I believe Prof. Pritchard's excellent observations are taken by his assistants, and when he finds they are perfectly discordant he throws away a whole night's work. My scientific grandfather, as I may call him, Col. Everest, taught his pupils, and they taught me, that you ought never to throw away an observation, if it was *bonâ fide* made, unless you have a good reason to give against it. But I am not prepared to say mere discordance under certain circumstances is not a sufficient reason to throw away an observation if it is presumed that there is a mistake made. I believe that a good deal of the difference in the discordances between the individual results as given by Prof. Pritchard and Prof. Pickering is, if I am right, the consequence of their different ways of dealing with results. It is marvellous to me that they are so accordant on the whole.

Lord McLaren. I should like to know what is meant by minimum visibility. I can understand extinction, but minimum visibility would give, I should think, a wide latitude to personality.

Mr. Ranyard. I must say the observation of Prof. Pritchard, that moonlight does not affect the determination of the brightness of stars, is very surprising, when we know how it does affect our estimate, both with the naked eye and a telescope, and that on a bright moonlight night we do not see stars of the third and fourth magnitude.

The Astronomer Royal. That is not a correct statement of fact, because it is not a question whether we see more or fewer stars by moonlight, but a question whether we shall extinguish the stars by the wedge photometer or by any other means at the same readings on a moonlight night as on a moonless night. The visibility of stars on a moonlight night is not concerned at all in this matter.

Mr. Howlett. I have been recently testing the candle-power of incandescent filaments in connection with electrical work, and I should like to know whether it is generally considered that the wedge should be moved rapidly or slowly. I found, practically, that when I brought the wedge to the point nearly approaching extinction, so that I could only just see the filament, I could see the filament longer by very slowly moving the wedge than by moving it rapidly; and I should imagine the same would be the case in the extinction of the star by Prof. Pritchard's method. With respect to the moonlight affecting the absolute point on the scale of the wedge when a star would disappear, it certainly does seem very marvellous to many of us, because if you carried out the principle logically to the bitter end, I do not quite see why you should not see stars by sunshine as well as by moonlight.

The President. We have had a most interesting discussion, and we are all much indebted to Prof. Pritchard for his paper, for which I am sure we shall all thank him. It is quite a miracle we have had this discussion to-night, because the paper only arrived while I have been sitting at this table.

The Rev. Mr. Saxby said he had hoped to have been able to exhibit an electrical observatory lamp, but unfortunately it had not arrived. He would, however, send an account of it to the 'Observatory.' It was a small incandescent lamp, supplied by a secondary battery, and arranged to hook on to the button-hole. Another thing he desired to mention to the Society. He was about to take a holiday in the High Alps, and had supplied himself with a spectroscope calculated to do good work, and ranging from A in the red to well below the H lines. He had arranged for a series of observations at an altitude of 10,400 feet, on the Schwarzhorn, and had already sent out a six-inch equatoreal to be used at an elevation of 5300 feet. He should feel glad if those who had paid attention to the spectroscope in high altitudes could supply him with any hints that would enable him to make his observations, as an amateur, of real use.

Mr. Green explained some drawings of Jupiter which he had made; and desired to call attention to some very remarkable spots following the old red spot. He had made one or two remarks about them in the 'Observatory.' The spots were seen following the red spot in the same latitude, and were apparently of the same kind. Of these he had made some drawings, and if they should enlarge to anything like the proportion of the red spots, it would be interesting to know how they started. They appeared to begin as a fine dark line, and then enlarged to a claret-coloured line. The larger one, which followed the old red spot at an interval of an hour and a half, was distinctly oval and red, rather deeper than the old red, but not so brilliant. It was surrounded by a line of light on the equatoreal side furthest from the south pole, similar to the old red made in his other drawings. He might say that for 20 years he had made careful drawings of Jupiter, and he hoped by degrees to know something about the question. The colour in the drawings now exhibited had been put on in the dark light of the Observatory, so that in the strong light it would appear a little exaggerated.

Mr. Todd. We have made some careful drawings of Jupiter in South Australia; and I hope, during my stay in England, I shall be able to submit them to the Society for what they may be worth. The dark spots which Mr. Green has referred to we saw very distinctly at Adelaide.

Mr. Ranyard. The great red spot seems to have a whitish patch in the middle, with a reddish line round the edge. There were similar drawings made 15 years ago in America. It seems to me that these spots have been spreading during the last two oppositions.

The President. I am happy to be able to announce that we have with us an old Fellow of the Society, and an old colleague of my own, who has only arrived in England two days ago. He has been thirty years absent from England, and is no other than Mr. Todd, Postmaster-General of South Australia and Government Astronomer of that Colony. We shall be very glad if Mr. Todd will come forward and give us some little account of the work they are doing in South Australia, particularly in connection with astronomy. (Applause.)

Mr. Todd. I did not expect to be called upon by the President to make any remarks with regard to our doings in South Australia. Having received an astronomical education in the Royal Observatory at Greenwich, I did not forget my first love. I had to struggle in the young colony with a great many difficulties, and for many years, although I was styled Government Astronomer, I did very little else than carry on, as far as I could with the small assistance and means at my disposal, meteorological observations. But I seized on the proper time, I think, for inducing the colony to make a start towards the establishment of an astronomical observatory, by availing myself of the transit of Venus in 1874. I pointed out that South Australia, as an English colony, should certainly do something towards the forwarding of astronomical science; and I recommended that an equatoreal should be purchased for the Observatory. This was done, and I succeeded in getting that instrument (an 8-inch refractor by Cooke and Sons) into good working order about one night before the transit of Venus occurred. I was pleased to know that my early attempt was successful. I believe that in the official reports the observations made by myself of egress (I was unfortunate in not being able to get ingress owing to the clouds) came very close to the truth when the whole observations over the world were taken into account. We have lately had a vexed question between South Australia and Victoria, as to where our boundary was. It was described in the Imperial Act as the 141st meridian. Recent surveys seemed to show that the boundary-line, which was determined in 1847, was considerably to the west, and therefore encroached upon the dominions of South Australia; and the Government asked me to determine where the boundary-line was. At the same time it became necessary to mark the common boundary-line of New South Wales and South Australia north of the River Murray. It was thought desirable that this line at least it should be marked out as correctly as possible. Towards rectifying the frontier between South Australia and Victoria we could do nothing. The Government asked me what I thought had best be done. At that time we were erecting a line of electric telegraph to connect Adelaide and Sydney directly by a line along the River Murray, independent of the line to Melbourne; and I recommended that voltaic signals should be exchanged between a temporary observatory near the boundary and the Melbourne and Sydney Observatories. I had no transit-instrument; but, fortunately, Mr. Ellery, the Astronomer of Victoria, had recently obtained a new transit-circle made by Simms, and at my request he very kindly placed his old instrument at my disposal. I so far encroached upon his kindness that I have retained that instrument to this day. It enabled me to make a commencement by starting transit-observations and giving true time to Adelaide and to drop a time-ball near Port Adelaide. Having succeeded in getting an equatoreal, I let the Government have no peace until they gave me a transit-circle, and they recently built a transit-circle room, which is about 34 feet long; and there are four other rooms used as offices and computing rooms. It is one of the finest transit-circle rooms in the southern hemisphere. I had only just before I came away succeeded in getting my transit-circle into good adjustment. There is a peculiarity in connection with that transit-circle to which I will draw the attention of the Society: it is virtually a large theodolite. That is to say, the bases upon which the piers rest consist of three parts: first, the sole-plate, bolted to the granite foundation; secondly, the plate which, by means of antagonistic screws, is adjusted in azimuth; and, thirdly, a frame by which the piers are levelled. I was obliged to resort to an expedient of that kind because of the very large earth-movements with which I had to contend. I may briefly say, the transit-circle is an instrument having an object-glass of 6 inches clear aperture, and the circles are 36 inches in diameter. It is a very splendid piece of work, and I should like to bear my testimony to the excellent work turned out by Mr. Simms. I may also refer to my success in observing the transit of Venus in 1882; for whilst it was cloudy at all the stations established in the Blue Mountains and along the east coast, I was favoured with a clear sky at Wentworth. Up to the present time, although I have the instruments I have described, I do not know that we have done any great astronomical work of much value. We have paid special attention to Jupiter, and have been successful in seeing some satellites of Jupiter through the edge of Jupiter itself. But a great deal of our work is confined to meteorology; we have a very complete system of observations; and I publish a weather bulletin with isobar maps. I keep six of these always exhibited, showing them for six succeeding days; and I think I have taught the people of Adelaide to predict the weather for themselves. They are posted, not only in the Adelaide General Post-Office, but also at Port Adelaide and other places where they are useful to shipping. They show all calculations of varying atmospheric pressure, not only in Australia, but in New Zealand; and they show in a most remarkable manner the progressive march from west to east of all areas of low pressure.

A vote of thanks was accorded to Mr. Todd, and the proceedings terminated.

The following papers were also announced:—

Prof. A. Hall. "Observations of the Satellites of Saturn, and of the Companion of Sirius, made at the United States Naval Observatory, Washington."

A. Marth. "Ephemerides of the Satellites of Saturn, 1885–86."

A. Marth. "Ephemeris of the Satellite of Neptune, 1885–86."

J. R. Hind. "On Daylight Occultations of Aldebaran in 1885."

The Rev. Thos. Perkins, M.A., was duly elected a Fellow of the Society.

The Meeting adjourned at 10 P.M.

Photometric Observations of Ceres ①*, Pallas* ②*, and Vesta* ④*, at the Harvard College Observatory.*

The asteroids Ceres and Pallas have recently been observed with the meridian photometer of the Observatory. In the following list, the successive columns contain the numbers of the series of observations, the date, the Greenwich mean time, the observed magnitude, the corresponding magnitude computed for mean opposition, after correction for scale and atmospheric absorption, the residual from the mean result, and the initial of the observer. The Greenwich mean time is that of the transit at Greenwich increased by the difference of longitude, as the planets were always observed very nearly at their culminations. On April 27 a star was observed by mistake for Ceres, and a similar error occurred on May 2 in the case of Pallas. The object observed was identified in both these cases by its recorded position, and the observations were accordingly omitted. The observation of Pallas on May 10 has been retained in the list, but the identification of the planet was not satisfactory. The result has not been included in the mean.

Ceres ①.

No. of	Date	G.M.T.	Magnitude.		Resid.	Obs.
Series.	1885.	h	Obs.	Red.		
441	Apr. 16	16·1	6·9	7·38	−·33	P.
443	Apr. 17	16·0	7·1	7·57	−·14	P.
445	Apr. 18	15·9	7·1	7·56	−·15	P.
447	Apr. 20	15·8	7·2	7·66	−·05	P.
454	May 2	14·8	7·5	7·86	+·15	P.
455	May 10	14·3	7·5	7·77	+·06	P.
456	May 11	14·3	7·4	7·66	−·05	W.
458	May 15	13·9	7·8	8·02	+·31	W.
460	May 16	13·9	7·7	7·90	+·19	P.
			Pallas ②.			
435	Apr. 6	15·5	7·5	8·58	+·03	W.
439	Apr. 14	14·9	7·5	8·47	−·08	W.
441	Apr. 16	14·8	7·6	8·54	−·01	P.
443	Apr. 17	14·7	7·7	8·62	+·07	P.
445	Apr. 18	14·7	7·7	8·60	+·05	P.
447	Apr. 20	14·5	7·7	8·57	+·02	P.
449	Apr. 23	14·3	7·6	8·41	−·14	P.
450	Apr. 25	14·2	7·7	8·69	+·14	W.
452	Apr. 27	14·0	7·9	8·65	+·10	P.
455	May 10	13·2	[7·1]	[7·67]	[−·88]	P.
456	May 11	13·2	7·8	8·39	−·16	W.

The mean result for Ceres from nine observations is 7·71 with a probable error of ·05; for Pallas, from ten observations, it is 8·55, with a probable error of ·02. The corresponding magnitudes, for a distance of unity from the Sun and from the observer, would be 4·27 and 5·10. The probable error is of course unaffected by this reduction.

Twenty-two observations of Vesta were made in 1880–82 with the smaller meridian photometer. The result is given in vol. xiv. of the 'Annals of the Observatory,' p. 410. The magnitudes of the planet for mean opposition and for the distance of unity were respectively 6·47 and 3·93. The probable error of the result is ·04.

E. C. Pickering.

On the Defining-power of Telescopes.

A parting word on the practical advantage of aperture. Mr. Denning is evidently under the impression that 18 inches can only be used to advantage on "rare" occasions. I have fortunately kept a notice on the side of my drawings of the aperture with which they were made, and going back over the drawings of Jupiter from the past three oppositions, which will give a fair average, I find as below :—

4	drawings on	3	evenings with	7	inches aperture.
9	„	7	„	9	„
13	„	9	„	11	„
31	„	21	„	13	„
75	„	49	„	15	„
55	„	36	„	18	„

showing a large majority in favour of the larger openings; but as on a good evening several views may be produced as against one or two drawings on a poor occasion, the most fair statement is the number of evenings on which the larger apertures were employed.

The favourite stop is evidently 15 inches, and I have no doubt that had the mirror been one of 15 inches, the favourite stop would have been 13. That is, supposing the curve of a mirror is in proportion to the length of its focus, the difficulty on a bad night is its outside portion, and by cutting this off the image is made more steady.

In all differences of opinion on observational astronomy, personality must have a large share. No one observer can be a rule to another, each will seek and finally adopt that which best serves his own power of vision, and reveals best those forms or appearances to which his attention is directed.

NATH. E. GREEN.

Harvard Photometry.*

IN the February Number of this Journal, in which attention is called to the contents of the first part of the fourteenth volume of the 'Annals of the Harvard College Observatory,' a reference is made to the second part of the same work, of which it was anticipated that the interest would not be less than that of the portion then under review. The publication of the entire work is now complete; and a careful examination of the whole will not only confirm the accuracy of this prediction, but will show that the reputation this observatory had acquired under the direction of Bond, Winlock, and Peirce has been sustained and extended.

The scheme of the director, assisted by a staff of unusual extent, has apparently not only embraced the production of a 'Uranometria' the most complete and the most elaborate that has yet appeared, but to collect within a moderate compass the results of all that is most valuable and interesting yet accomplished in this branch of astronomy. This could only be done by adopting methods of printing which have sometimes sacrificed clearness to brevity.

* Annals of the Astronomical Observatory at Harvard College, Vol. xiv. pt. 2.

Broadly stated, the main feature of the second part of the Harvard Photometry is the comparison of the original measures of magnitude, given in the first part, both with other photometric determinations and with the eye estimates of various observers, and of the subjects which grow naturally out of this comparison. It surveys the results and discusses the accuracy, or at least the agreement with the Harvard observations, of every important catalogue from Ptolemy to the present day. Considering the comparative newness of such investigations from a practical point of view, and the important cosmical results which it is hoped may ultimately be obtained from such inquiries, an impartial intercomparison of any existing observations must possess considerable value; but it is necessary to consider with some care those points which may affect the impartiality of the inquiry or tend to make the conclusions misleading, either in fact or in appearance.

It is evident that eye estimations, admittedly rough and expressed by a notation like that employed by Argelander or Herschel or others, which does not lend itself readily to numerical calculation, must be treated differently to rigorous photometric measures, expressed by the various observers in a precise form. And it must be remembered, especially of the first class of observations, that the final comparison applies not so much to the observations themselves as to the interpretation put upon them by the investigator. For example, an original observer may have left it on record that a star shone with a magnitude which he expresses by a symbol 2·3; and if on examination by another observer and a particular method, this symbol is translated to mean 2·48, then the probable error theoretically computed on that assumption is simply the error which would belong to that catalogue, if every star so described had had that decimal originally attached to it. The alteration once made, the subsequent calculations no longer strictly refer to the catalogue as left by the original authority.

The method adopted by Prof. Pickering to effect such an alteration is as follows:—First, the mean photometric magnitude was obtained for each group of stars, to which the observer attached the same designation, either fractional or integral. Employing the entire magnitudes as abscissæ and the corresponding photometric means as ordinates, a continuous curve was drawn among points so projected, and the points on the abscissa line corresponding to the photometric mean for the fractional magnitude were read off. A mean was then taken of the various fractions that corresponded to the similarly described deviations from the integral magnitudes, that is to say, for 1·2, 2·3, 3·4, &c.; and the finally adopted magnitude for descriptions such as these was the point on the photometric curve corresponding to that fraction on the abscissa line. The process is somewhat involved, and may not admit of description in a few lines without some obscurity; but it will be easily seen that, to some extent, an agreement is forced between the Harvard magnitudes and the catalogue so treated.

On applying this method to Ptolemy's Catalogue, the main residual from the standard is 0·35 mag.; and after making some, though possibly an inadequate, allowance for the fact that Ptolemy's Catalogue has been compared with the Harvard rather than with the true magnitudes of the stars, this probable error is reduced to 0·29 mag., which suggests an appearance of accuracy not possessed by the catalogue itself.

But possibly this importation of the Harvard results into the work and catalogues of other observers is carried to its greatest extent in the case of Sir W. Herschel. If we look at the list of residuals given in the first part of the photometry, we find a column devoted to Herschel, and from the numerous residuals given in that column we might conclude that an extensive numerically expressed catalogue had been or could be independently constructed from that astronomer's work. But Sir W. Herschel's astrometric researches consisted in the formation of numerous short sequences of only two or three stars in each, the difference of whose magnitude, always small, is indicated by certain symbols. The determination of the numerical value of these symbols forms a legitimate and interesting inquiry, which has been very efficiently conducted by Prof. Pickering, though it had already been accomplished by other astronomers, and the mean values are set out with great detail and clearness. The next step consists in assuming the magnitudes of the comparison-stars from the Harvard Catalogue, and applying to them the mean value of the symbols previously obtained. The result is, of course, in a great measure the reproduction of the Harvard Catalogue itself, subject to the discrepancies necessarily occasioned by assigning a uniform or mean value to Herschel's symbols.

The examination of Herschel's sequences has been the subject of very evident care, and the difficulties which those sequences present have possibly been effectually met, but some 250 discordant results, of which there is no sufficient explanation, are left outstanding. It frequently happens in Herschel's work that the same star occurs in more than one comparison, and then several sequences can be connected together. This desirable step does not seem to have been taken by Prof. Pickering, or the number of the discordances and contradictions would have been materially increased. Another difficulty frequently met with in arranging these longer sequences is, that the continual addition of the differences of magnitude, indicated by the symbols, eventually assigns to the stars at the end of the series a far greater numerical magnitude than can be consistently attached to them. The possibility of such results indicates discordances in the originals which it is to be feared cannot be effectually encountered. Only those who have been engaged in similar inquiries know the labour and care involved and the uncertain scientific value of the final results obtained.

The comparison with photometric catalogues does not admit of much variety of treatment; but the results are not the less curious or valuable. The catalogue of Wolff, in which the measures of light are expressed in logarithms, will illustrate the method generally pursued. The arithmetical complements of the logarithms given by Wolff are divided by 0·4, and a series of residuals is formed by subtracting the magnitude so obtained from those given in the Harvard Catalogue. This step by itself constitutes an independent comparison between the catalogues; but, on effecting it, a systematic difference depending upon the magnitude is at once evident to a very considerable amount, and the harmony of the two catalogues can apparently be effected only by applying to that of Wolff a series of corrections which are in some sort a function of the magnitude. The probable error of Wolff's magnitude is then stated to be only ·09; but we submit that this is not the probable error of Wolff's catalogue, but of Wolff's catalogue as modified by Pickering.

It is not intended to imply that any step of the process is concealed or disguised; but unless this attempt to remove systematic differences at the outset is thoroughly understood and admitted, the important table on page 502 may mislead. No scientific purpose is served by minimising, even in appearance, the defects of any series of observations. It is a regrettable circumstance that the careful and conscientious work of Wolff and others requires a correction drawn from the results of another instrument, that it has been so corrected, and that the errors of the original catalogues, as they stand, are not more prominently exhibited. In the list of the errors of catalogues already referred to, only those of Peirce and Pritchard present the actual discrepancies from the Harvard Catalogue unaltered.

For the Harvard Catalogue itself, the probable error, as given, is smaller than that found for any catalogue with which it has been compared. This assumption of accuracy will not be allowed to pass unchallenged; and it is probable that the increase in the theoretical probable error, to which Peirce's catalogue has had to submit, will be repeated in the present instance, when these results have, in their turn, undergone a close and detailed examination. Even if this should unhappily prove the case, it will affect but little the value of the Harvard volume in its entirety, which, from the mass of information with which it is accompanied and the collection of facts by which it is distinguished, has deserved the thanks of all who are engaged in this department of astronomy.

It was intended to offer some remarks upon other important questions discussed in the volume, such as the existence and treatment of discordant observations, cases of suspected variability of stellar brightness, the photometric observation of planets, the distribution of the stars, &c.; but for these subjects the reader must be referred to the volume itself. WILLIAM E. PLUMMER.

University Observatory, Oxford,
1885, June 20.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Changes in the Length of the Great Red Spot on Jupiter.

Gentlemen,—

The following table is constructed from observations of the Red Spot made with the 5-inch refractor, the magnifying-power generally being 173. The observations of May 12, 1885, were made with the 6-inch Cook equatoreal and a power of 200.

The lengths are obtained by taking the difference between the observed transits across Jupiter's central meridian of the *p.* and *f.* ends of the spot, and multiplying this interval in minutes by 0·604, that being the fraction of a degree of longitude on Jupiter that will pass a meridian in one minute of time, supposing the rotation period to be that of the Red Spot.

It seems quite evident from these observations that the spot attained a maximum length some time in the first part of October 1880.

Though the observations are but eye-estimations, they have been carefully made, and I think truly indicate changes in the length of the spot.

Observed Length of the Great Red Spot on Jupiter.

Date.	Length.	Remarks.
1880.	°	
July 10 ..	30·2	From middle and *f.* end. Good.
17 ..	26·6	*P.* and *f.* single observations.
24 ..	27·5	do. do.
29 ..	32·6	do. do.
31 ..	30·2	do. do.
Aug. 13 ..	33·8	Image good.
17 ..	27·8	
23 ..	27·9	*P.* and *f.* ends single observations.
28 ..	29·0	From middle and *f.* end.
30 ..	29·9	*P.* and *f.* ends single observations.
Sept. 9 ..	28·7	do. do.
16 ..	31·4	do. do.
18 ..	29·6	do. do.
25 ..	28·6	*P.* and *f.* each the mean of 3 observations.
28 ..	29·3	do. do.
30 ..	31·7	do. do.
Oct. 1 ..	33·0	do. do.
6 ..	33·8	do. do.
7 ..	33·1	do. do.
10 ..	33·2	do. do.
13 ..	31·3	do. do.
20 ..	28·9	do. do.

Date.	Length.	Remarks.
1880.	°	
ov. 1 ..	31·2	*P.* and *f.* each mean of 3 observations.
4 ..	29·2	From middle and *f.* end.
8 ..	30·9	*P.* and *f.* each mean of 3 observations.
10 ..	29·7	do. do.
11 ..	30·9	do. do.
18 ..	31·0	do. do.
20 ..	30·0	do. do.
22 ..	30·0	do. do.
23 ..	29·0	*P.* single est., *f.* mean of 3 observations.
ec. 2 ..	27·2	*P.* and *f.* each mean of 3 observations.
5 ..	26·8	do. do.
7 ..	28·4	From *p.* end and middle, each mean 3 obs.
9 ..	28·9	*P.* and *f.* each mean of 3 observations.
14 ..	29·1	do. do.
31 ..	29·2	*P.* mean of 3, *f.* single observation.
1881.		
an. 7 ..	27·1	*P.* and *f.* each mean of 3 observations.
ar. 6 ..	26·5	*P.* mean of 3 obs., *f.* single observation.
uly 9 ..	27·1	*P.* and *f.* each mean of 3 observations.
16 ..	25·7	From *p.* end and middle, each mean 3 obs.
21 ..	27·4	*P.* and *f.* ends each mean of 3 observations.
28 ..	26·1	*P.* and *f.* each mean of 3 observations.
ct. 14 ..	26·6	*P.* and *f.* single observations.
ov. 5 ..	27·2	do. do.
29 ..	26·0	*P.* and *f.* single estimates.
1882.		
an. 23 ..	26·6	*P.* and *f.* single observations.
1885.		
ay 12 ..	29·0	Difficult. *P.* and *f.* single observations.

anderbilt University Observatory,
shville, Tenn., U.S.A., 1885, May 25.

E. E. Barnard.

Bayer's Lettering of the Constellations.

ENTLEMEN,—

An erroneous impression has widely prevailed, and is still be found in many even of the most modern works on astronomy, t Bayer, in affixing Greek letters to the principal stars in the erent constellations, arranged them strictly in the order of alphabet. Thus in Prof. Ball's well-known 'Elements of tronomy,' we read (chapter 3), "To the brightest star of the stellation the letter α was assigned, the next brightest is denoted β, the next by γ, and so on throughout the alphabet." An [illegible] Bayer's Explicatio sufficiently indicates that such [illegible] him. This was pointed out by Argelander in his academical Dissertation entitled "De Fide Uranometriae Bayeri," published at Bonn in 1842, and mention has also been recently made of it by Dr. Gould in his 'Uranometria Argentina.' [illegible] Bayer divides the stars in each constellation [illegible] classes of magnitudes, according to the ordinary principle. [illegible] The stars of the highest order of magnitude in each [illegible] that number of the first letters of the Greek [illegible] their number of the next following [illegible] attempt was made to arrange the stars [illegible] respective magnitudes, or rather, as [illegible] the stars of each order, the sequence [illegible] represented that of their brightness, [illegible] the position of the stars in the figure, beginning [illegible] the course until all the stars of [illegible] were exhausted." Or, to quote the earlier [illegible] these litera prior majore [illegible] vulgo creditur, sed ibi ord [illegible] stellae ejusdem classis adjacente [illegible] designentur, ordoque a capit [illegible] ejus tractum sequatur." There wa [illegible] this, as was pointed out by Arago in hi [illegible] viz., that Bayer did uniforml [illegible] star in each constellation by the letter α [illegible] part of the imaginary figure it was in: and it wa probably this which has led to the ordinary and erroneous notio about the arrangement.

The error in question has led to some other suppositions which, when it is pointed out, are at once seen to be unfounded. Thus in Mr. Chambers's well-known and valuable 'Handbook of Descriptive Astronomy' we read (3rd edition, p. 479), "It should be remarked that, owing either to carelessness on the part of Bayer, or to changes in the brightness of the particular stars, or to both causes combined, this alphabetical arrangement in many cases does not accurately represent the relative brilliancy of the different stars in a constellation." In like manner, under "Astronomy" in the new (ninth) edition of the 'Encyclopædia Britannica,' after a statement that, in Bayer's 'Uranometria,' "each of the stars in every constellation is marked by a letter of the Greek alphabet, commencing with the most brilliant, which is designated by α; the next most conspicuous is called β, the third γ, and so on," we read, "Moreover, whether owing to carelessness in the original lettering or to changes in the brightness of the stars, the order of the letters does not always correspond to the brightness, even in the same constellation." From the above, it is evident that no such conclusion as either of these can be drawn. Perhaps one of the most remarkable instances in which the lettering is seen at a glance not to follow the order of the letters, is that of the three brightest stars in Aquila, γ being evidently brighter than β. But there is no occasion to conjecture from this that any change of relative brightness has taken place. Bayer reckoned both these two of the second magnitude, and appears to have arranged β before γ according to his usual custom, simply because β is in the neck of the supposed eagle, and γ at the root of one of the wings.

Yours faithfully,

Blackheath, 1885, May 13. W. T. Lynn.

Halley's Comet of 1717.

Gentlemen,—

The eminent contemporary of Newton and the first to detect the periodicity of the fine comet of 1682 appears himself to have discovered a small comet in 1717, though I find no mention of the fact in Chambers's 'Descriptive Astronomy.' In recently looking through some very old numbers of the 'Ladies' Diary' (which contain many records of eclipses and other references to astronomical observations, which are, I believe, chiefly extracted from the Phil. Trans.) I came across a passage in the number for 1717 as follows :—"On Monday June 10th [old style] of this year Dr. Halley discovered a small telescopical comet, whose place at 11 o'clock that evening was ♐ 17° 12′ with 4° 12′ south latitude." Is any thing further known of this comet? In 1713 Halley became Secretary of the Royal Society and in 1720, on the death of Flamsteed, was appointed Astronomer Royal.

The same publication I have been quoting says :—"In this year [1718] from the 18th of January to the 5th of February, new style, a comet was observed at Berlin by Mr. Christ. Kirk." Then follow the positions on 11 nights and further remarks. In Chambers's catalogue I see the discovery is attributed to Kirch, who, I suppose, is the same as the Godefroi Kirch who on Oct. 26, 1699, had observed a faint comet in Argo visible to the naked eye, and with a motion sensibly southwards, but not redetected on subsequent nights. If Christ. Kirk, the discoverer of the comet of 1718, was a different person to the Godefroi Kirch referred to in Chambers's, the latter is wrongly mentioned in connection with the discovery of 1718.

If Mr. Lynn or Mr. Chambers could give us any information on these several points, I feel assured it would be very interesting.

Yours faithfully,

Bristol, June 4, 1885. W. F. Denning.

Alteration of Bye-Law 12 *of the R. A. S.*

Gentlemen,—

I beg to correct an error in your report of the Special General Meeting of the Royal Astronomical Society on the 8th inst.

The exact wording of the alteration of Bye-Law 12, finally handed in by Lord Crawford and agreed to by the meeting is :—

[illegible] addressed and posted to the Scrutineers of the Ballot [illegible] present at the Meeting, shall be accepted if duly [illegible] the signature of the absent Fellow on the envelope."

I am, [illegible], your obedient servant,

E. B. Knobel,
Secretary R. A. S.

[illegible]
[illegible] V.
[illegible]

NOTES.

The Harvard College Observatory.—Prof. Pickering has [illegible] Report for the year [illegible]. We regret to find that [illegible] of the scientific work [illegible] inevitable. It has been found [illegible] with the services of five assistants, and [illegible] in expenditures of other kinds. [illegible] the restriction of the activity of the Observatory [illegible] temporary, and that through th[illegible] the present difficulties [illegible]

The [illegible] work of the year was of a very varied and [illegible] character. The [illegible] observations of the eclipses [illegible] continued as in past years. The [illegible] given in Volume vi. of the [illegible] during the year. The charts of [illegible] right ascension and [illegible] in [illegible] bright stars, have [illegible] series of investigations with [illegible] spectra [illegible]. It has been [illegible] have banded spectra with [illegible] the positions of the bands [illegible]. The method of measurement [illegible] spectrum with a notched bar beside [illegible] the telescope. The proper posi[illegible] reference to an image [illegible] pass beside the prism which [illegible] proposed to observe all stars to [illegible] of the thirtieth parallel. [illegible] designed for investigating [illegible] of the star to be observed is [illegible] same means as in the other [illegible] then carried by its diurnal motion [illegible] placed at right angles to the [illegible] are accordingly visible in the [illegible]. The successive extinction of [illegible] is observed in a wedge of tinted [illegible] of the Observatory, however, will probably not permit the completion of these observations upon the original plan. The meridian circle has also been actively employed during the year, as well as the meridian photometer, with which 141 series of observations have been taken. The principal investigation now carried on with the latter instrument consists of the measurement of stars, the magnitude of which has been estimated at each of the two observations during the recent revision of the Durchmusterung. The number of such stars is about eight thousand. Under the head of "Miscellaneous," Prof. Pickering mentions the observations of variable stars which have been carried on at Harvard College, in co-operation with other observers in the United States. It would indeed be a misfortune to our science should the operations of the Observatory be restricted in the way in which Prof. Pickering appears to think is inevitable, and we hope before long he will find himself in a position to prosecute the various researches referred to in this Report, with such assistance as may be considered necessary.

SUSPECTED VARIABLE STARS *.—Mr. J. E. Gore, already well known for his labours in this department of Astronomy, has published a catalogue of Suspected Variable Stars, which will doubtless prove most useful to amateurs engaged in observations of this interesting class of objects. The catalogue has been compiled from various sources, including many observations made by the author himself both in India and (since October 1877) in Ireland. Mr. Gore's observations were made with a binocular *accurately focussed*, he having found this plan preferable to that of putting the glass slightly out of focus, as is the habit of some observers. Dr. Gould also expresses himself in favour of the method of accurate focussing. It is considered, however, that the observations hitherto made of these stars have not been sufficient to place their variability beyond doubt. The magnitudes which have been assigned to each star by different observers are given in the notes; and a glance through these brings prominently before us the great service which Dr. Gould has rendered to this branch of our science by the publication of the 'Uranometria Argentina,' a work which is of course the chief authority on stellar magnitudes and their variations in the southern hemisphere. We notice that throughout the paper the author writes 'Ophiucii' instead of 'Ophiuchi,' and this occurs so often that it seems impossible to regard it as a misprint. The catalogue, which contains altogether the places of 773 stars reduced to 1880·0, is accompanied by a map showing the distribution of known and suspected variables, and forms a very desirable supplement to the author's 'Catalogue of known Variable Stars.'

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* Proceedings Royal Irish Academy, 2nd ser. Vol. iv. no. 3.

STANDARD SOUTHERN CIRCUMPOLAR STARS.—In the Introduction to Vol. v. of the Cordoba Observations, Dr. Gould gives a new determination of the places of the 54 circumpolar stars which have been used for determination of azimuth-error of the transit-circle during the progress of the observations for the zones and for the general catalogue. He considers that the time has now arrived when these positions can be determined with considerable accuracy, and the computed value of the probable error of each determination shows how fully these determinations may be relied on. Although the resultant positions are not quite identical with those used in reducing the zone observations, yet their close agreement with the latter is sufficient to show that there are no sensible errors in the Cordoba results due to inaccuracy in the adopted azimuthal deviations of the instrument. The definitive positions have been deduced from all the available observations of each star, including those of Lacaille, as reduced by Powalky or by Henderson's tables, Johnson, Maclear, Taylor, Gilliss, Ellery, Stone, and the Cordoba results, down to 1882 inclusive for the right ascensions, and to 1883 for the declinations. For each star is given the final position for 1870·0 and the corresponding proper motion, together with the values of the coefficients for referring the place to the Equinox of any other date $1870+t$, by the formula

$$a=a_0+A\,.\,t+B\,.\,t^2+C\,.\,t^3+D\,.\,t^4+\;.\;.\;.\;.\;.\;.\;.\;\&c.,$$

with a similar expression for the declination. These are doubtless the most accurate positions of the southern circumpolar stars which we possess, and their publication is another and very great service rendered to our science by Dr. Gould. We presume that they will be adopted in the various astronomical Ephemerides.

OBSERVATIONS OF VARIABLE STARS IN 1884*.—In this paper Prof. Pickering gives a concise but clear view of the progress of observations, in Europe as well as in America, of this interesting class of objects during the year 1884. The following observers have co-operated with Harvard College in this important branch of astronomical research, viz.:—Backhouse and Knott in England; Dunér, Hartwig, Safarik, and Wilsing on the Continent of Europe; Eadie, Hagen, Parkhurst, and Sawyer in the United States. Prof. Pickering gives a list of about 150 variables which have been observed during the year, with the number of nights on which each star was observed by the astronomer whose designation is attached to the number. It is hoped that observers of variable stars will continue to furnish accounts of their work during each year as soon as possible after its close. It is desirable that these accounts should be received at the Harvard College Observatory as early as February 1 of the following year.

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* 'Proceedings American Academy of Arts and Sciences,' Vol. xx. p. 393.

ASTRONOMY IN CHRISTIANIA.—A very laudable effort at teaching the general public practical astronomy is being made in Christiania. An optician, Herr A. Olsen, has erected a great refractor in the Royal Park—in size said to be the fifth in the world—through which the celestial bodies can be observed by the public for a small fee, while explanations are given of their nature &c. The interior of the pavilion in which it is mounted is hung with celestial charts and diagrams, as well as views of the planets, the Sun, and the Moon, for the purpose of facilating the object in view. The cost of the instrument was nearly £2000.

UNIVERSAL TIME.—The Lords Commissioners of Council of Education have requested the following committee to advise them what steps should be taken with respect to the proposed change of time:—Prof. J. C. Adams, The Astronomer Royal, Capt. Sir F. Evans, R.N., The Hydrographer of the Navy, Gen. Strachey, Dr. Hind, and Col. Donnelly. In accordance with their recommendation copies of the Report of the Delegates to the Washington Conference, and the resolutions adopted, have been sent to various departments of State, to the Royal, Royal Astronomical, and Telegraph Engineers Societies, the Railway Clearing House and various Telegraph Companies. My Lords state that these resolutions of the Conference appear to them such as commend themselves for adoption, but before so informing the American government they would be glad to receive the opinion of the various bodies.

THE OXFORD UNIVERSITY OBSERVATORY.—The Annual Report of the Savilian Professor of Astronomy to the Board of Visitors of the Observatory was read on June 3. From it we learn that the Professor's lectures both in theoretical and practical astronomy have been well attended by the students. In addition to the ordinary subjects of lecture, it is proposed hereafter to introduce that of the theory of probability, and also to give two public lectures on the somewhat numerous and curious passages in Polybius containing allusions to astronomy, and which bear more or less on the application of astronomy to chronology. Prof. Pritchard has been able to complete the construction of a subsidiary observatory for educational purposes. It contains the Simms Altazimuth belonging to the observatory and also a telescope of 4½-in. aperture mounted equatoreally and with clockwork motion. With regard to the original astronomical work carried out at the University Observatory, Prof. Pritchard states that the memoir on the evidences of mutual gravitation among the components of the Pleiades group has been published by the Royal Astronomical Society. The completion is also announced of the photometric survey of the relative lustre of all stars visible to the unaided eye from the pole to 10° south declination. A small portion of this investigation has been published in the 'Memoirs' of the R.A.S.; it is proposed that the complete work should be printed by the delegates of the University Press. Prof. Pritchard also refers to his examination of the relative capacity of the two large telescopes* for the transmission of light, a subject upon which he is still engaged. It must be conceded that the Savilian Professor has made excellent use of the opportunities which have been afforded him for original research, and has explored fields which have hitherto been too much neglected in this country.

The Harvard College Catalogue of 1213 Stars for 1875.—An unexpectedly large number of typographical errors having bee discovered in the first issue of this Catalogue ('Observator Vol. [illegible]), a second edition has been printed and distribute in which all the errors hitherto found have been corrected.

The Leander McCormick Observatory.—This Observator attached to the University of Virginia, was opened by publi ceremony on April 13. As most of our readers are aware, thi Observatory possesses an equatoreal refractor of 26 inches apertur the gift of Mr. Leander McCormick, from whom the institutio takes its name. The directorship of the Observatory, held b Prof. Ormond Stone, is endowed with a sum of £10,000 collecte by public subscription; while Mr. W. H. Vanderbilt has given a further endowment of £5000. Prof. A. Hall gave the openin address on "The Instruments and Work of Astronomy."

A Correction.—In No. 98, p. 222, we stated that Prof. E Schonfeld had discovered two new variables. Really only the firs star is variable, the second, a small star 10·8 mag., being used a a comparison star.

Floating Domes.—'Ciel et Terre' claims the credit of the firs idea of these domes for a Belgian engineer, M. Royers, who lai such a project before the Minister of the Interior in 1880.

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* These are a refractor of 12¼ in. aperture and a silver-on-glass reflector of 13 in. aperture.

Royal Observatory, Brussels.—M. F. Folie, administrating inspector of the University of Liège, has been appointed director of this Observatory. Since the resignation of M. Houzeau, the direction has been in the hands of a committee of three, MM. Stas, Liagre, and Mailly.

A New Minor Planet (No. 248) was discoveved on June 5 by Dr. J. Palisa of Vienna.

The Annual Meeting of the 'Astronomische Gesellschaft' will be held this year at Geneva, and will commence on Wednesday, August 19.

Maxima and Minima of Variable Stars in July and August 1885.

M, signifies maximum ; *m*, minimum.

July	5	R Canis Minoris, *m*.
	7	S Cephei, M.
	7	R Coronæ, *m*.
	8	U Monocerotis, *m*.
	8·4	T Monocerotis, *m*.
	9·6	β Lyræ, *m*.
	10	R Lyræ, M.
	11	U Herculis, M.
	12	S Delphini, *m*.
	12	S Vulpeculæ, M.
	12	R Ursæ Majoris, M.
	13	S Canis Minoris, *m*.
	13	U Capricorni, M.
	14	V Cancri, M.
	14	T Arietis, M.
	16	T Herculis, *m*.
	16·4	T Monocerotis, M.
	20	R Herculis, M.
	22	S Libræ, M.
	22·5	β Lyræ, *m*.
	24·3	W Virginis, *m*.
	26	T Delphini, M.
	27	L² Puppis, M.
	28	T Aquarii, *m*.
	29	R Capricorni, M.
Aug.	1	U Monocerotis, M.
	1·5	W Virginis, M.
	2	R Aurigæ, M.
Aug.	4	W Scorpii, M.
	4·4	β Lyræ, *m*.
	4·4	T Monocerotis, *m*.
	5	R Arietis, M.
	5	R Lacertæ, M.
	6	S Arietis, M.
	7	R Vulpeculæ, *m*.
	10	R Lyræ, *m*.
	10·5	W Virginis, *m*.
	11	R Coronæ, M.
	12·4	T Monocerotis, M.
	13	S Ursæ Majoris, *m*.
	13·3	ζ Geminorum, M.
	17	V Coronæ, M.
	17	R Boötis, *m*.
	17·3	β Lyræ, *m*.
	18	S Sagittarii, *m*.
	18·4	ζ Geminorum, *m*.
	18·7	W Virginis, M.
	19	R Comæ, M.
	19	R Piscium, M.
	20	S Vulpeculæ, *m*.
	23	U Monocerotis, *m*.
	23·4	ζ Geminorum, M.
	25	R Lyræ, M.
	28	S Ophiuchi, M.
	28·6	ζ Geminorum, *m*.
	29	T Cassiopeiæ, M.
	31·4	T Monocerotis, *m*.

Variables of Short Period.

δ Cephei.

	d.
July	2·35, *m.*
	7·65, *m.*
	9·35, M.
	14·65, M.
	18·45, *m.*
	25·45, M.
Aug.	3·55, *m.*
	10·55, M.
	19·65, *m.*
	26·55, M.
	30·35, *m.*

Algol, m.

		h	m
July	5	11	46
	25	13	28
	28	10	17
Aug.	14	15	11
	17	12	0
	20	8	48

W Sagittarii.

	d.
July	6·4, M.
	18·4, *m.*
	21·6, M.
Aug.	2·6, *m.*
	13·4, M.
	25·4. *m.*
	28·6, M.

U Sagittarii.

	d.
July	9·6, *m.*
	12·6, M.
Aug.	5·6, *m.*
	8·5, M.

η Aquilæ.

	d.
July	23·4, M.
	30·6, M.
Aug.	4·4, *m.*
	11·6, *m.*

U Cephei, m.

		h	m
July	4	13	33
	9	13	13
	14	12	52
	19	12	32
	24	12	11
	29	11	51
Aug.	3	11	30
	8	11	10
	13	10	49
	18	10	28
	23	10	8
	28	9	47

λ Tauri, *m.*

		h	m
Aug.	12	15	45
	16	14	37
	20	13	29
	24	12	22
	28	11	14

S Cancri, m.

		h	m
July	3	11	10

δ Libræ, m.

		h	m
July	5	9	38
	12	9	12
	19	8	46
	26	8	20
Aug.	4	15	45
	11	15	19
	18	14	53
	25	14	27

U Coronæ, *m.*

		h	m
July	13	14	8
	20	11	51
	27	9	33
Aug.	13	15	49
	20	13	32
	27	11	14

Astronomical Memoranda, July and August 1885.

Equation of Time (Sun *after* Clock).

		m	s			m	s
July	1,	3	34	Aug.	1,	6	2
	11,	5	13		11,	4	56
	21,	6	8		21,	2	53
	31,	6	6		31,	0	5

Sidereal Time at Mean Noon.

		h	m	s			h	m	s
July	1,	6	38	49	Aug.	1,	8	41	
	11,	7	18	14		11,	9	20	
	21,	7	57	40		21,	9	59	
	31,	8	37	5		31,	10	39	

Sun.		Rises. h	Rises. m	Sets. h	Sets. m	Position-angle of axis. °	Position-angle of axis. ′	Heliogr. co-ordinates of centre of disk. Lat. °	Lat. ′	Long. °	Long. ′
July	5	15	53	8	15	359	29	+3	34	55	18
	10	15	58	8	13	1	46	4	5	349	8
	15	16	3	8	8	4	1	4	34	282	59
	20	16	9	8	4	6	14	5	1	216	49
	25	16	15	7	56	8	22	5	26	150	40
	30	16	24	7	49	10	26	5	49	84	33
Aug.	4	16	31	7	42	12	25	6	9	18	26
	9	16	39	7	33	14	18	6	27	312	19
	14	16	47	7	23	16	5	6	43	246	13
	19	16	54	7	14	17	44	6	55	180	9
	24	17	3	7	3	19	17	7	5	114	4
	29	17	11	6	53	20	41	+7	11	48	1

Planets for July 1885.

net.	R.A.	Dec.	Diam.	Rises.	Tr.	Sets.
	h m s	° ′	″	h m	h m	h m
:ury..	7 3 23	24 17 N.	5·0	16 8	0 25	8 45
	8 28 45	20 54 N.	5·4	17 17	1 11	9 8
	9 36 3	15 17 N.	6·0	18 21	1 38	9 1
	10 26 36	9 10 N.	7·0	19 1	1 49	8 39
us ..	7 50 12	22 22 N.	10·2	17 5	1 11	9 20
	8 41 32	19 49 N.	10·4	17 34	1 23	9 14
	9 30 52	16 21 N.	10·6	18 7	1 33	9 2
	10 18 10	12 10 N.	10·9	18 36	1 41	8 46
s	4 29 13	21 48 N.	5·0	13 45	21 49	5 56
	4 58 57	22 49 N.	5·0	13 28	21 40	5 53
	5 28 36	23 29 N.	5·1	13 13	21 30	5 48
	5 58 1	23 48 N.	5·2	13 1	21 20	5 40
iter ..	10 19 17	11 35 N.	31·0	20 35	3 40	10 43
	10 25 53	10 56 N.	30·3	20 5	3 7	10 7
	10 32 56	10 14 N.	29·8	19 35	2 35	9 31
	10 40 20	9 30 N.	29·4	19 6	2 3	8 55
irn ..	5 57 27	22 31 N.	14·7	15 6	23 15	7 28
	6 13 26	22 31 N.	15·3	13 24	21 33	5 48
nus ..	11 57 49	1 2 N.	3·6	23 5	5 18	11 27
	12 1 31	0 36 N.		21 13	3 24	9 31
tune..	3 31 7	17 18 N.		13 13	20 49	4 29
	3 33 51	17 27 N.		11 17	18 54	2 35

Planets for August 1885.

:cury..	10 30 47	8 34 N.	7·0	19 4	1 50	8 36
	11 2 41	3 20 N.	8·2	19 20	1 42	8 2
	11 11 12	0 41 N.	9·8	18 56	1 11	7 18
	10 49 19	2 56 N.	10·6	17 52	0 10	6 29
ıus ..	10 22 47	11 43 N.	11·0	18 33	1 42	8 44
	11 8 13	6 58 N.	11·4	19 12	1 48	8 26
	11 52 31	1 54 N.	11·8	19 42	1 53	8 6
	12 36 19	3 16 S.	12·2	20 13	1 57	7 45
rs	6 0 57	23 49 N.	5·2	13 0	21 19	5 39
	6 29 56	23 46 N.	5·2	12 50	21 8	5 29
	6 58 20	23 24 N.	5·3	12 42	20 57	5 15
	7 26 1	22 44 N.	5·5	12 35	20 46	4 59
ırn ..	6 13 55	22 31 N.	15·3	13 21	21 30	5 44
	6 26 51	22 25 N.	15·9	11 36	19 45	3 58
tune..	3 33 55	17 27 N.		11 13	18 50	2 31
	3 34 46	17 28 N.		9 16	16 53	0 34

[illegible] Mercury at greatest elongation 27° 21′ E.
[illegible] Mercury stationary.
[illegible] Neptune stationary.

The Moon.

	Rises.			Sets.			Rises.			Sets.	
	h	m		h	m		h	m		h	m
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Aug. [illegible]	[illegible]	[illegible]	Aug. 17	10	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	18	11	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	19	12	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	20	12	49
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	21	13	41
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	22	14	38
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	23	15	37
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	24	rises	
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	25	6	47
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	sets		26	7	12
[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	27	7	37
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	28	8	2
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	29	8	28
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	30	8	56
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	9	[illegible]	31	9	30
[illegible]	[illegible]	[illegible]				[illegible]	[illegible]	[illegible]			

[illegible] Last Quarter. Aug. 3d 9h 55m Last Quarter.
[illegible] New Moon. [illegible] 14 New Moon.
[illegible] First Quarter. 17 1 47 First Quarter.
[illegible] Full Moon. [illegible] 5 25 Full Moon.

[illegible] August 1885.

Day.		Star.	Mag.	Disap.		Angle.	Reap.		Angle.
				h	m	°	h	m	°
[illegible]	[illegible]	B.A.C. [illegible]	[illegible]	14	37	[illegible]	15	59	297
	18	[illegible] Virginis	[illegible]	[illegible]	[illegible]	[illegible]	10	27	179
	[illegible]	[illegible] Libræ	[illegible]	[illegible]	[illegible]	[illegible]	12	1	236
	[illegible]	[illegible] Ophiuchi	[illegible]	[illegible]	31	17	11	1	335
Aug.	[illegible]	B.A.C. [illegible]	[illegible]	15	32	169	15	56	214
	[illegible]	B.A.C. [illegible]	[illegible]	11	[illegible]	137	12	35	238
	[illegible]	B.A.C. [illegible]	[illegible]	12	8	84	13	15	291
	[illegible]	[illegible] Sagittarii	[illegible]	12	35	128	13	39	256
	[illegible]	[illegible] Aquarii	5	17	9	114	18	9	286

The R.A. and N.P.D. of Planets and the Solar Co-ordinates are given for Greenwich Mean Noon: in all cases Greenwich Mean Time is used, reckoning from noon to noon, as in previous years; the angles for Occultations are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 100. AUGUST. 1885.

Biela's Comet, the Meteor-shower of November 27, 1872, *and Comet f* 1881 (*Denning*).

Towards the end of November of the present year the circumstances appear to be extremely favourable for a recurrence of the slow meteors from Andromeda, which formed such a very fine display in 1872, and which had been previously observed by Brandes on December 7, 1798, by Paul Flaugergues on December 6, 1838, and by Heis at Aix-la-Chapelle on December 8, 1847. If Biela's duple comet still exists in any considerable degree of condensation, and if it has preserved nearly the same orbit as during the years 1826–52, it should be in perihelion only a short time before the Earth crosses the node, and the conditions for a meteoric display are even more auspicious than in 1872; but the question is whether planetary perturbations have so disturbed the orbit as to materially alter the periodic time, and otherwise modified the elements as to render the meteor-shower no longer visible, at least in its best aspect, from the Earth. There was no sign of the shower in 1879, and the ensuing November ought to definitely settle the question as to its continued existence. For my own part I do not believe it can possibly recur, notwithstanding the circumstances are so good, my impression being that the orbit was disturbed in 1872; and it seems a fair conjecture that *the comet discovered at Bristol on the morning of October* 4, 1881, *was really the meteor-cloud through which the Earth passed on the night of November* 27, 1872, and that meteor-cloud itself is admitted to have represented the remains of the apparently lost comet of Biela.

As the epoch of the expected meteor-shower is now drawing near, the question of its appearance and the fate of Biela's comet will be much discussed, and I am anxious to mention the facts which have led me to suggest the identity of this remarkable comet with that discovered at Bristol.

1. On the night of November 27, 1872, the number of meteors was so great, that there is no doubt the Earth was involved with the train of Biela's comet not far from the nucleus. The comet

[illegible]

[illegible]

[illegible]

4. [illegible] December [illegible] N. of the Earth's [illegible]. It [illegible] ascending node [illegible] remembering [illegible] descending node, though the positions are as different as they well can be. This singular agreement of the nodes was also noticed by Mr. W. E. Plummer in his [illegible] ('Copernicus,' January 1883) of the Comet *f* 1881, though he seems to ascribe it to an accidental circumstance. He says — "It is curious to remark that the longitude of the ascending node of this comet corresponds almost exactly with that of the descending node of Biela's comet." This latter longitude is given by Dr. Hubbard (for the mean equinox of 1852) as 65° 51′ 25″. Mr. Plummer gives the ascending node of Comet *f* 1881 as 65° 52′ 2″·04 (mean equinox of 1881), and adds: "Of course no great importance can be attached to this similarity of longitude, for if the number of known comets be increased sufficiently, some such coincidences must of necessity be observed. It adds, however, another point of interest to this particular comet."

5. The period of Comet *f* 1881, according to Mr. Plummer, is 3235 days, and Dr. Hartwig's, published in the Astr. Nach. No. 2405, is almost identical, viz. 8·8335 years, or 3226 days. The perihelion passage occurred on September 13, 1881. Now I cannot

but regard it as a most significant fact that the interval between the great meteor-shower of November 27, 1872, and the perihelion date of Comet *f* 1881 is 3211 days, agreeing almost exactly with the computed period of revolution of the latter. And if the perihelion of Comet *f* 1881, as derived from the orbit assigned by Mr. Plummer, occurred a few weeks prior to its nodal passage in November 1872, as it unquestionably did, then the close character of the agreement is even more striking.

6. It is impossible to conceive that Comet *f* 1881 being so bright could have escaped observation before its return in 1881, had it been observable under the same condition of orbit and aspect. Either Tempel, Winnecke, or Coggia would most certainly have discovered it. Mr. J. Ritchie says ('Science Observer,' vol. iii. p. 94):—"Denning's comet could have been seen here [Boston, U.S.] a month or more before discovery, being then more favourably situated and brighter. In the southern hemisphere it must have been visible as early as July, and on August 18 it was within 11,000,000 miles of the Earth, and its brilliancy was equal to forty or fifty times that at discovery, and in fact easily visible to the naked eye." Mr. Hind gives an ephemeris in 'Nature,' vol. xxv. p. 567, from which it appears that on August 4 and 6, 1881, the comet was rapidly travelling northwards among the stars of Argo Navis, and was fully 28 times as bright as on the date of its discovery. It is evident from this that the comet is a very conspicuous one and comes extremely near the Earth; "Biela's comet alone approaches closer to the Earth's orbit." We are therefore justified in assuming that the path of the comet as derived from observations in 1881 was a new one, and that it could only be referred to the action of planetary perturbations upon some previously known comet, resulting in a great change of orbit. Upon no other thesis can we satisfactorily account for the fact of its non-detection in previous years.

7. The aphelia of the short-period comets are grouped near the orbits of the larger planets of our system. The feature is very decided, though perhaps not satisfactorily accounted for. Comet *f* 1881 offered a distinct departure to the rule, and "indicated a mean motion differing considerably from that of any comet yet observed." This anomaly of orbit coupled with its small inclination would also suggest that the comet had been recently passing through some vicissitudes and disturbances from one or other of the planets which it may have closely encountered.

To summarize the matter as briefly as possible, I believe that when Biela's comet encountered the Earth on November 27, 1872, it must have been violently disturbed and the orbit enlarged to the extent of some 800 days in the periodic time, the average of the four revolutions between the perihelion of February 27, 1827, and September 23, 1852, being 2434 days, whereas the period of Comet *f* 1881 is some 3235 days. And the perturbation must have been such as to bring about a reversal of the nodes. If these changes were impressed upon the chief condensation of Biela's meteor comet, then it would certainly appear that the comet which I first saw in Leo on October 4, 1881, was really the representative of the great meteoric display of November 27, 1872, observed at Bristol and at many other stations in England and abroad.

The fact of the coincidence in the nodes in Long. 65°, and in the period elapsed between the meteor display and the perihelion of Comet *f* 1881, with the computed period of the latter from the observations between October 5 and November 19 of that year, coupled with the circumstance that no such orbit had been deduced for any comet previously observed, is very suggestive, and I trust the question of identity may now be critically investigated. Pogson's comet of December 2 and 3, 1872, discovered near θ Centauri and supposed to have represented the receding meteor group, lends additional interest to the subject.

The perihelion passage of Biela's comet at its first-recorded apparition in 1772, when it was discovered by Montaigne occurred on February 19 of that year, and its average period during the 12 revolutions which it performed up to the last observed return in 1852 (perihelion September 23) was 2453 days. If planetary perturbations further retarded the period a few days (making it about 2465 days) during the three following returns, which were unobserved, the comet would have been close to its descending node as the Earth crossed it on November 27, 1872, and the great meteor-shower is at once explained. A review of the conditions of such an encounter would suggest considerable disturbance of the comet's path, and the probability that the orbit of Comet *f* 1881 may have been imposed upon it, or, rather, upon one of its parts.

In presence of the doubts respecting Biela's double comet and our ignorance as to the exact extent of the perturbations to which it may have been subject, it is impossible to say whether there will be any prominent meteoric display next November. According to Prof. Newton, who discussed the several showers of 1798, 1838, and 1872, it would appear that the meteor-stream of this comet is distributed along at least 500,000,000 miles of the orbit; but this conclusion is based on the assumption that the comet had passed its perihelion early in September 1872, which is extremely doubtful and certainly not supported by observed facts. Moreover the displays of 1798 and 1838 were trivial in comparison with that of 1872. In 1798, December 7, Brandes, observing near Hamburg in Germany, relates that 400 meteors were counted in a few hours in half of the sky. Now two observers are necessary to command half the heavens; and adopting three hours as the interval of observation, and allowing 12 meteors per hour as belonging to minor contemporary showers, we get an horary rate of about 55 Andromedes for one observer. In 1838, December 6, the shower was described by M. Bouvy, assistant at the Observatory at Brussels. Meteors were four times as numerous as on an ordinary night, so that the hourly number (deducting sporadic meteors) would be about 36*. In 1847, December 8, Heis witnessed the shower, but it was by no means a rich one, for in 5½ hours he counted 152 meteors, of which about one fifth were Andromedes, so that the rate per hour was about 6. These several displays were incomparable with the grand phenomenon of November 27, 1872. Between $5^h\ 50^m$ and $9^h\ 15^m$ that night Mr. E. J. Lowe estimated that, in a fourth part of the sky, 13,470 meteors fell. This is equal to some 3940 per hour for one observer!

Whether this system was so disturbed by contact with the Earth on the latter occasion as to have suffered considerable modification of orbit must be settled by further observations. If there is no sign of the meteors at the important time in November next, we could hardly have stronger evidence that some such change has really occurred. W. F. DENNING.

Bristol, June 1885.

APPENDIX.

MY DEAR SIR,—

Your letter, received this morning, was so interesting, I set to work at once to examine the matter more closely. The coincidence is even more striking than you suppose.

In the enclosed diagram I have drawn, roughly, the portions of the orbits of Biela and of your comet which lie near perihelion projected on the plane of the ecliptic, *i. e.* the plane of the paper. The point occupied by the Earth Nov. 27 is marked at 66° of heliocentric longitude, and as the descending node of Biela's Comet.

	Denning (1881 *f*).	Biela 1852.
T..	1881 Sept. 13.	Sept. 28.
π	18° 36′	109° 8′
☊	65 52	245 51
π − ☊	47 16	223 17
π − ☋	....	43 17
i	6 50 (direct).	12 33 (direct).
log q	9·86090	9·93480
e	0·83041	0·75592
P..	3235^d = 8·8562 Jul. year.	2418^d = 6·6197 Jul. year.

The long. of the perihelion of your comet is 19° and the perihelion distance ·726. The inclinations of both, being small, are neglected.

23·7 days after P.P. your comet would arrive at its ascending node, at which time it would be only about ·136 from the Earth (and somewhat nearer to Biela's node) towards the Sun. This would make it in its ascending node on Oct. 7, that is 3236 days after 1872 Nov. 27. Plummer's period is 3235 ± 10 days! It is probable that on the 27 Nov. 1872 your comet was very near the Earth, *and mixed up with the meteoric showers*!

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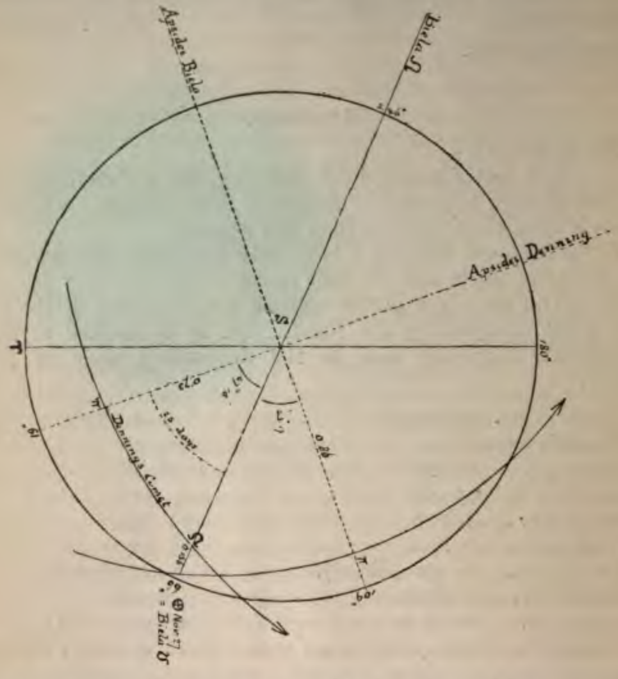
* The apparent excess of shooting-stars on this night and on Dec. 7, 1798, and Dec. 8, 1847, may have been due to active showers of Geminids and (ζ) Taurids, common to this epoch, and which usually have the effect of raising the horary numbers to an abnormal figure.

If we suppose that one of the constituents of Biela's Comet passed so close to the Earth on that day as to have its orbit violently disturbed (that is, if its inclination was altered 19° or 20°, its mean motion retarded as 9 : 6 about, and its line of apsides advanced in longitude 90°), it would become identical with your comet. The reversal of the node is a necessary consequence of altering the inclination of Biela more than 13° through zero.



The attraction of the Earth could only be relatively very great for a few hours, owing to the great velocity of the comet while overtaking the Earth, so that it is very doubtful if so great a disturbance could take place. In 1874 your comet remained for many months near Jupiter, and must have been much perturbed. We know for certain that the Earth produces no sensible disturbing effect upon the meteors of Nov. 27, because their radiant is identical with the Biela cometic radiant. That, however, proves nothing in point; because the meteor-paths are directed so trul to the centre of the Earth (the actual radius of the Earth being only ·000045, and this would be the maximum distance for meteors 90° from their radiant) that the Earth's attraction would only slightly accelerate their speed, not bend their tracks.

The difficulty about Biela having "touched the Earth" on 1872 Nov. 27 is that if it did so it was 12 weeks behind its supposed place. As far as I know, no one has yet computed the perturbations between 1852 September and 1872 November. It may turn out that the comet might have been retarded 12 weeks in the five complete revolutions. Pogson's Comet remains a stumbling-block, for if Biela's Comet had the new orbit imposed upon it and was immediately brought up to the north side of the plane of the ecliptic, Pogson's Comet would have been similarly acted upon. Pogson's Comet, however, had not certainly passed *very near* the Earth, and therefore might not have been so much perturbed. His comet might have been sufficiently perturbed to appear "off the track" of Biela. It is altogether an extraordinary problem.

Yours very truly,
G. L. Tupman.

W. F. Denning, Esq.

College Road, Harrow-on-the Hill.

Comet-Meteor Radiants.

Below will be found a list of Comet-Meteor Radiants and Distances which I have calculated from the orbit elements of recent comets.

The present list includes the radiants of all the principal comets that have appeared since the first of 1880. The only ones omitted are some three or four faint ones of short period which have already been included in previous lists.

The first column contains the current number; the second gives the designation of the comet by the recent method in the order of perihelion passage of the year; the third, the discoverer; the fourth, the day of the Earth's passage through plane of comet's orbit; the fifth designates the nearest node; the sixth, the distance of this node from the Earth's orbit in the order $R-r$, the Earth's distance from the Sun being taken as unity; the seventh and last column gives the radiant-point for the given date.

If, as is generally accepted, comets are formed of meteors, or, at least, have meteors travelling in the same orbits with them, we may be able to identify some past or future showers with some of the comets of the last few years, which has been a period of so much cometary activity.

Allowing for the lateral spreading of meteors in their orbits, we see there are several comets whose meteors we should be liable to encounter, the most noteworthy case being that of Comet 1882 I., where the distance from the Earth at node is less than three million miles.

Current Number.	Designation of Comet.	Discoverer.	Day of Earth's passage through line of Nodes.	Designation of Node.	Earth's distance from Node.	Comet-Meteor Radiant.	
						R.A.	Dec.
1.	1880 I.	Gould.	Mar. 16	Descending.	+0·98	298°·1	− 6°·2
2.	1880 II.	Schaeberle.	Dec. 9	,,	−1·02	194·6	+28·9
3.	1880 III.	Hartwig.	Nov. 7	Ascending.	+0·60	142·0	− 8·0
4.	1880 IV.	Swift.	Jan. 17	Descending.	−0·68	305·6	−12·2
5.	1880 V.	Pechüle.	May 31	Ascending.	+0·33	44·7	−74·6
6.	1881 II.	Swift.	July 30	Descending.	+0·43	347·3	+69·7
7.	1881 III.	Tebbutt.	June 22	Ascending.	+0·28	56·4	−58·8
8.	1881 IV.	Schaeberle.	June 28	Descending.	+0·20	14·7	+29·7
9.	1881 V.	Denning.	Nov. 28	Ascending.	+0·12	274·1	−36·1
10.	1881 VI.	Barnard.	June 26	,,	+0·57	19·9	−34·8
11.	1881 VIII.	Swift.	Sept. 24	Descending.	−1·66	116·9	+39·9
12.	1882 I.	Wells.	Apr. 15	Ascending.	+0·03	356·9	−14·4
13.	1882 II.	Gould.	Mar. 6	Descending.	+0·97	290·9	− 8·1
14.	1882 III.	Barnard.	Dec. 1	,,	−0·51	137·8	+56·9
15.	1883 I.	Brooks.	Dec. 30	,,	−0·14	244·0	+25·6
16.	1883 II.	Ross.	Dec. 16	,,	+0·63	175·8	+42·0
17.	1884 I.	Pons Brooks.	Dec. 6	,,	+0·18	198·9	+67·9
18.	1884 II.	Barnard.	Sept. 28	Ascending.	−0·69	198·5	−17·2
19.	1884.	Wolf.	Oct. 20	Descending.	−0·58	275·2	+39·7

Harvard College Observatory,
1885, June 26.

O. C. WENDELL.

Notes on some of Flamsteed's Stars.

the 'Philosophical Transactions' for 1783, Sir W. Herschel kes some remarks with reference to stars which he considered d probably changed in lustre since Flamsteed's time. A few marks on some of these supposed changes may be of interest.

(1) Sir W. Herschel says, "ι Delphini is much larger than κ" ough marked by Flamsteed of a less magnitude). In his imates of relative magnitude, however (Phil. Trans. 1796 and 99), he estimated ι just perceptibly less than κ, and equal to Delphini. ι has been suspected of variation by Franks, and is . 633 of my Catalogue of Suspected Variable Stars, where rther details will be found. In August 1875, I found ι about 1 gnitude fainter than κ; but in August 1876, August 1877, and tober 1879, ι only slightly less than κ.

(2) Herschel says:—"δ Canis Majoris is larger than β, and yet marked to be less." In the Harvard Photometry δ is 1·85 and 2·01, or only 0·16 m. in favour of δ. The estimates (2·3 and 2·2) ke δ 1 step *fainter* than β. At Cordoba δ was estimated 2·1 and 2·2 ('Uranometria Argentina,' pp. 196, 197), so that modern timates make the stars very nearly equal in brightness. Flameed's low estimate of δ was probably due to its great southern clination. In January 1876 and February 1877, in Northern dia I found β slightly superior to κ Orionis (2·22 H.P., and 2·3 A.). δ is a suspected variable (No. 208 of my Catalogue).

(3) Herschel says: "δ Delphini is larger than ε, though placed in inferior order." δ was rated 3–4 by Sufi, and ε 4–3. They ere both rated 4 m. by Argelander and Heis, and both 3–4 by ouzeau. The Oxford measures make ε (3·56) about one magnide brighter than δ (4·59), while the Harvard measures make the fference less than half a magnitude in favour of ε (4·13 and 4·57). hese discordant estimates seem to show variation in one or both ars. In August 1876 I found "ε slightly brighter than δ."

(4) Herschel says:—"The 1st and 2nd Hydræ are noted by Flameed as being of the 4th magnitude, whereas they are now only of e 8th or 9th. It is remarkable that the 30th Monocerotis, which is tuated between them, has retained the order assigned to it by Fl, d being of the 6th magnitude serves to point out the change of e other two in a very evident manner." Now 1 and 2 Hydræ ere estimated 6·2 at Cordoba, so that Herschel's estimate of their agnitude was as much below their present brightness as Flameed's was above it. 30 Monocerotis, called C Hydræ by Gould, was timated 3·8 at Cordoba. It was measured 3·63 at Oxford and 86 at Harvard. It has been suspected of variation by Franks, ho rated it only 5 m. in January 1878, and Herschel's estimate of m. seems only explicable on this hypothesis. My own observaons in 1884 vary only from 3·8 to 4·0, but the period may be one f many years.

(5) With reference to stars in Draco, Herschel says:—"The

change in the magnitudes of the 31st and 34th Draconis is very striking, these two stars being just the contrary of what they are marked in Flamsteed's Catalogue. The 31st from the 7th is increased to the 4th; and the 34th from being a star between the 4th and 5th is reduced to one of the 6th if not the 7th magnitude." These stars are ψ^1 and ψ^2 Draconis respectively. ψ^1 (31) was rated 4·5 by Heis, and was measured 4·84 at Oxford and 4·52 at Harvard. Gemmill suspects slight variation. It is a wide double star. ψ^2 (34) was measured 5·56 at Harvard, or about 1 magnitude less than ψ^1.

(6) Herschel says:—"The 62nd Arietis is of the 5th magnitude, though only marked of the 6th." It was rated 6 m. by Harding and Heis, and was measured 5·59 at Harvard. The Harvard measure shows that the star might pass for a bright 6th. It is 6 m. in Harding's Atlas.

(7) Herschel says:—"The magnitudes of the 12th and 14th Lyncis are just the reverse in the heavens to what Flamsteed has marked them. This denotes a double change of a star from the 5th to the 7th, and from the 7th to the 5th." Both Harding and Heis give 12 as 5 m., and 14, 6 m. 12 was measured 4·68 at Harvard, and 14, 5·39; so that if Flamsteed's observations were at all correct, some change must have taken place in the relative brightness of these stars.

(8) Herschel says:—"The 38th Persei, marked of the 6th magnitude, is increased so as to be equal to θ and κ of the 4th. Also θ is less than τ, contrary to Flamsteed." 38 (o) was measured 4·47 at Oxford, and 4·01 at Harvard; θ 4·26 at Oxford, and 4·24 Harvard; κ 4·08 Oxford, and 3·95 Harvard; and τ 4·07 at Oxford, and 3·97 at Harvard. Here the only difference worth noticing is in the magnitude of 38 (o). It was rated 3–4 by Sufi, 4 m. by Argelander and Heis, 4–5 by Houzeau, and 4·8 in the 'Durchmusterung,' so that perhaps some variation may be suspected.

(9) Herschel says:—"ξ Leonis in Flamsteed's time was of the 4th; but it is now less than a star of the 5th magnitude." This star has been suspected of variation (No. 288 of my Catalogue). It was rated 6 m. by Sufi and Argelander, 5–6 by Heis, and 5·4 at Cordoba, and it was measured 5·2 at Harvard. My observations in 1884 vary only from 5·5 to 5·7.

(10) Herschel has some notes "On stars newly become visible." He says:—"Near Lacerta's tail end is a star of between the 4th and 5th magnitudes, not mentioned in Flamsteed's Catalogue, though the 1st Lacertæ, not far from that place, is recorded. It is so easy to be seen with the naked eye, and in a spot where but few of that magnitude are near, that we can hardly account for its being omitted if it had been visible to Fl. Its colour is pale red." This star seems to be identical with No. 607 of Birmingham's Catalogue of Red Stars, which has been suspected of variation by Secchi (No. 683 of my Catalogue). It is 7765 B.A.C. and was rated 5 m. by Heis, and measured 4·65 at Harvard. The observations in Birmingham's catalogue vary from 4·5 to 7·5. I estimated it 5·3 on

October 8, 1879. This star seems worthy of further observation, as it was probably faint at the date of Flamsteed's observation.

(11) Herschel says:—"A considerable star in a direction from the 68th Geminorum towards the 61st is not to be found in Fl, its colour is red." This seems to be Lalande 14620 (6 m.). It is 6 m. in Harding's Atlas, 6 m. Argelander and Heis, and it was measured 5·57 at Harvard (H.P. 1415). It is not in Birmingham's Catalogue.

(12) Herschel says:—"A star of considerable magnitude preceding the 1st Equulei is not contained in Flamsteed's catalogue. It is a double star of the first class, the 61st of my second collection, where measures of it will be found." This seems to be Σ 2735 = LL 40444–5 (7 m.). It was rated 6–5 by Heis, and was measured 5·90 at Harvard (H.P. 3677).

(13) Herschel says:—"A considerable star following the 1st Sextantis and another following the 7th are not inserted." With reference to the first of these, the only star which seems to suit Herschel's description is a 6 m. star shown in Harding's Atlas about 2° following 1 Sextantis. This star is not in Lalande's Catalogue, but is identical with DM. 7°, 2181 (=3336 B.A.C.), rated 6 m. by Argelander and Heis, 6·3 at Cordoba, and measured 5·98 at Harvard (6·2 estimated). The light would therefore seem to be constant or nearly so; but it is worthy of remark that closely south following this star, M. Borelly has recently discovered a star variable from 6 to 9 m. I cannot identify the second star referred to by Herschel "following the 7th;" but about 1° *north following* 7 Sextantis, Harding has a 7 m. star marked with a line under it, which is not in Lalande's Catalogue. I failed to see any trace of this star with a binocular field-glass on the following dates:—1877, February 18 and May 14; 1878, March 9; and 1885, February 14.

(14) Herschel says:—"Between β Cancri and δ Hydræ is a very considerable star, not marked by Flamsteed, though its situation is very remarkable. As the constellation of Cancer contains so rich a collection of very small stars, it is to be wondered how a star of such consequence could be omitted, if it had been visible in Fl's time." This seems to be Lalande 16530 (6 m.) = 2822 B.A.C., rated 6–5 by Heis, and 5·7 Gould, but measured 5·09 at Harvard.

(15) Herschel says:—"Nearly 1½° north following δ Herculis, almost in the direction of δ and ν, is a star of the 5th, or between the 4th and 5th, magnitude, very visible to the naked eye. We can hardly think Fl could have overlooked it had it been there in his time." I cannot identify this star unless it be Lalande 31581–2 (5½ and 6 m.), rated 6 m. by Argelander and Heis, and measured 5·36 at Harvard (H.P. 2902). It is 6 m. in Harding's Atlas.

(16) Herschel says:—"About 3° south preceding γ Bootis a considerable star not in Fl's catalogue of the 6th magnitude, and south preceding λ another almost as large." The first of these is apparently A Bootis=4747 B.A.C., 6 m. Harding, 5 Heis, and measured 4·77 at Harvard (H.P. 2409). The star *sp.* λ seems to be Lalande 25963–4 (5½, 4½)=4699 B.A.C., rated 6–5 by Argelander and Heis, and measured 5·47 at Harvard. J. E. Gore.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Red Spot on Jupiter.

GENTLEMEN,—

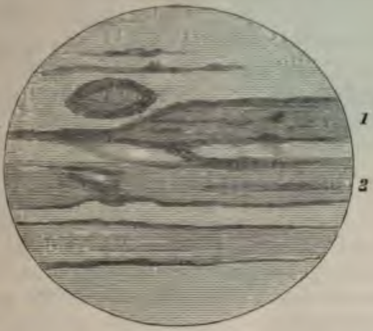
After reading the remarks of Prof. Hough of the Dearborn Observatory (Report for 1884), and the criticism of Mr. W. F. Denning on the same ('Observatory,' No. 95), also the paper of Prof. C. A. Young ('Observatory,' No. 97), and comparing all with my own experience in observing markings on Jupiter, and especially in observing the Red Spot, I am more than ever convinced that the atmospheric conditions under which we make our observations *are*, or *ought to be*, as potential in the formation of our judgments of *what we see* as the telescope itself.

The first observation of the Red Spot noted in the Northern Hemisphere I made here, July 9, 1878. During the seven intervening years, I have made numberless observations and notes upon it; but during 1884 my observations were less frequent and more unsatisfactory, from the *very long continuance* of poor atmospheric conditions. When the seeing improved I was preoccupied with other work.

Having of late turned attention again to the spot, and the seeing late in April and in May being very good, I am greatly surprised at the *sharp* and *distinct* outline which it presents. From accounts I had read of it in 1884–85, I was prepared to see a mere ghost of its former self. But while its redness is much lessened, its perfect outline and environment are entirely unimpaired. It is true that the central part is either *faded* to white, as seems implied by Mr. Denning's remark ('Observatory,' No. 95, p. 95), or else is *obscured* by an intervening white stratum, as seems implied by Prof. Young's remark ('Observatory,' No. 97). But even this "obscuration" varies much with the seeing; the better the seeing the more distinct and symmetrical is the interior white ellipse.

But with due deference to all the weight of authority which Mr. Denning brings to bear on the connection of the spot with the belt south of it in 1884, I must say that no such connection exists *now*. What may have been the appearance in 1884, under the best atmospheric conditions, I cannot say; I only observed it a few times during the opposition of 1884, and then the seeing was far from good. My best observations for that opposition were March 14 and March 26, when drawings were made. On March 14 the belt south of spot was so *far removed* that no thought of any connection could occur. On March 26 there is noted "a cloudy diffusion" from the belt towards the spot, which possibly is the "ligament" spoken of by Mr. Green, or the "arm" spoken of by Mr. Offord. But in my drawing this diffusion does not reach the spot.

But, on account of poor seeing, I cannot attach much importance to any observations or drawings made by me in 1884. I, however, observed the spot April 26–28, and on May 3, 7, 8, 10, 12, and 22, and made a *special study* of its connection with belts both south and north. The clear white border between the spot and the south side of the equatoreal belt admits of no dispute. But the narrow belt on the south side of the spot approaches *much nearer* to its rim. This belt is variable in tint, and sometimes is so dark as to appear as bluish black. It does not, however, touch the spot. True, when the seeing is only moderately good, or under very low powers, or with our 4-inch equatoreal, it *seems* to touch it; but in a good atmosphere, and with the 12¼-inch equatoreal and a power of 275, there is a fine white border running all round it, and completely isolating it from all belts. On the night of May 3, after satisfying myself on this point, and without stating to him my motive, I asked my son (who has had much experience in observing) to scrutinize the environment closely, especially on the south side. He did so, and stated that there was a clear line of white running all round the *south* border, as well as round the north. At the same time be recognized the interior white oval of which Prof. Young speaks, and also a darkening at the extreme point of the *following* end. After the transit of the central meridian he made a drawing of the spot and its environment, which I send with this paper. It presents the appearance at 14^h 10^m G.M.T.



(*Notes made on Drawing.*—Spot very light pink, about colour of belt 2, and entirely light in centre. Belt 1 Indian red. Following end of spot dark at point.—C. W. P., jun.)

I am sure, were I to look at Jupiter only through our 4-inch equatoreal (though it is an excellent instrument), I should think the spot and belt were in actual contact; but with the larger instrument I can have no such judgment. For this *difference* of judgment with the two instruments, I think Prof. Young clearly points out there is an "*optical necessity.*" I may state, further, that these observations were made on the very best nights for seeing, when every detail of the planetary surface stood out boldly and distinctly.

But I must certainly agree with Mr. Denning as to *sudden* and *astonishing* changes on the Jovian surface. I was examining Jupiter a great deal about the time the Red Spot first manifested itself, in July 1878. The surface from night to night was undergoing changes almost revolutionary, and I *still think* the Red Spot itself was of sudden formation, and at *first* had a large proper motion. Observers will remember that at the beginning of 1882 the whole structure of the equatoreal belts had changed, so that there were *three* broad equatoreal belts in place of *two*. On January 22, 1882, a fine sharp belt made its appearance between the middle and southern equatoreal belt. I had never before seen a belt in this position, though I had examined the planet hundreds of times in previous years. In the 'Kansas City Review,' for February 1882, I published some account of these changes.

It may not be amiss to publish the following times of transit of the Red Spot across the central meridian. The observations were made as described in Astr. Nach. No. 2294.

	G.M.T.	G.M.T.	G.M.T.
1885, Apr. 28.	....	Centre $13^h\ 54^m$	Foll. end $14^h\ 27^m$
May 7.	Prec. end $15^h\ 55^m$	Centre 16 26	
May 10.	Prec. end 13 30	Centre 13 59	Foll. end 14 27 ·5
May 12.	Prec. end 15 9	Centre 15 40	
May 22.	....	....	Foll. end 14 21 ·5

Very respectfully yours,
C. W. PRITCHETT.

Morrison Observatory,
Glasgow, Mo., 1885, May 26.

The Total Eclipse of May 12th, 1706.

GENTLEMEN,—

A recent reference, in a popular periodical, to the earliest observation (in the year 1706) of the so-called red flames in a total solar eclipse, having led me to consult the accounts of that eclipse in the 'Philosophical Transactions,' perhaps the description given by Dr. J. J. Scheuchzer (in a letter to Mr. Petiver, F.R.S.) of his observations at Zürich may be of interest to some of your readers. It will be remarked that he speaks of the eclipse as being quasi-annular as well as total, in consequence of seeing this brilliant red appearance all round the Moon's limb:—

"Illustri Societati indica, habuisse nos die 12° Maii, Eclipsin Solis totalem simul et annularem ; totalem, quoniam Sol integer à Lunâ fuit obtectus ; annularem autem non propriè ita dictam, sed per Refractionem, quandoquidem circa Lunam fulgor apparuit rutilans, à radiis per Atmosphæram Lunæ refractis ortus."

The eclipse was not total in England. There is a singular misprint as regards the date in Flamsteed's account ; he gives it as "May 1^{st} St. N. ;" but the eclipse occurred on May 12th New Style, and May 1st is the corresponding date of Old Style, then still used in England. The account sent to Flamsteed by Capt. Stannyan from Berne, where he was staying with his kinsman, then Her Majesty's Envoy to Switzerland, is well known; in it he states that the Sun's "getting out of the Eclipse was preceded by a Blood-red streak of Light, from its Left Limb, which continued not longer than 6 or 7 Seconds of Time;" upon which Flamsteed remarks:—"The Captain is the first man I ever heard of that took notice of a Red Streak of Light preceding the emersion of the Sun's body from a total Eclipse. And I take notice of it to you, because it infers that *the Moon has an Atmosphere*; and its short continuance of only 6 or 7 Seconds of Time, tells us that *its height is not more than the* 5 *or* 6 *hundredth part of her diameter.*"

Blackheath,
1885, June 12.

I am, Gentlemen, yours faithfully,
W. T. Lynn.

Brightness of β Aquilæ—A Correction.

Gentlemen,—

Allow me to correct an inadvertence in my letter to you of the 13th of May, printed in your number for the present month. In the last line but two (p. 247), for "second" read "third." Bayer reckons α Aquilæ of the second magnitude, and all the stars from β to λ of the third.

My argument of course merely went to show that for the stars in each scale of magnitude, Bayer followed, in the sequence of the letters, the form of the figure, not the smaller gradations of brightness; so that no conclusion could be drawn, for instance, from β preceding γ in the alphabet, that β Aquilæ was brighter than γ Aquilæ in his time. Whether it may really have been so or not is quite another matter. It is well known that the former star has sensibly diminished in brightness within the memory of persons living; but no change has taken place in this respect for many years past, nor is there any reason to suppose that it was ever brighter than γ. Ptolemy reckoned both of the third magnitude, and Bayer, as usual, simply followed the old distribution, recognizing only integral divisions of magnitude. Flamsteed rated β Aquilæ as 3½; whilst Hevelius, remarkably enough, estimated it as only (what it is now) the fourth, although later observers reckoned it, like Flamsteed, of the 3½ magnitude.

Yours faithfully,
W. T. Lynn.

Blackheath, 1885, July 8.

The supposed Comet of 1717 *and the Comet of* 1718.

Gentlemen,—

In answer to Mr. Denning, I beg to say that an account of Halley's observation of the supposed comet seen on the 10th O.S. (21st N.S.) of June 1717 is given in the 'Philosophical Transactions,' and is quoted by Pingré. But the object was only seen on that one night, when it was very near the planet Mars; and it is so exceedingly doubtful whether it was really a comet, that it seems to me Mr. Chambers was quite right not to mention it in either of the lists in his 'Handbook.'

Gottfried Kirch, who discovered the second comet of 1699 (mentioned in Chambers's second list, as it was not observed sufficiently for any determination of its orbit), died in 1710. Christfried Kirch, who discovered the comet of 1718, was the son of Gottfried; he was born in 1694 and died in 1740.

Yours faithfully,
W. T. Lynn.

Blackheath, 1885, July 3.

P.S.—As some of your readers may not have easy access to early volumes of the 'Philosophical Transactions,' it will perhaps be acceptable to them to quote Halley's account of the object seen on June 10, 1717, which he afterwards believed to be a comet. It is as follows:—

"On *Monday*, *June* 10 in the Evening, the Sky being very serene and calm, I was desirous to take a View of the Disk of *Mars* (then very near the *Earth*, and appearing very glorious) to see if I could distinguish, in my 24 Foot Telescope, the Spots said to be seen on him. Directing my Tube for that purpose, I accidentally fell upon a small whitish Appearance near the Planet, resembling in all respects such a *Nebula* as I lately described in *Philos. Transact.* No. 347, but smaller. It seemed to emit from its upper part a very short kind of Radiation, directed towards the East, but Northerly withal; which, considering its situation, was nearly towards the Point opposite to the Sun. The great Light of the Moon, then very near it and also near the Full, hinder'd this *Phænomenon* from being more distinctly seen; but its Place in the Heavens was sufficiently ascertained from the Neighbourhood of *Mars*, from whom it was but about half a Degree distant towards the Southwest." After stating that there were two small stars near it, and that he, with two friends, contemplated the appearance for considerably more than an hour without being able to trace any motion in it, leading them to conclude that it was rather a nebula than a comet, Halley proceeds:—

"However, suspecting that it might have some Motion, I attended the next Night, *June* 11th, at the same Hours and in the same Company, when with some Difficulty by reason of the Thickness of the Air, we found the two little Stars, but the *Nebula* could not at that time be seen, which we then imputed to the want of a clearer Sky. But on *Saturday*, *June* 15, the Moon being absent, and the Air perfectly clear, we had again a distinct View of the two Stars, with an entire Evidence that there remained no Footstep or Sign of it, in the place where we had first seen this *Phænomenon*, which we therefore now found to be a Comet, and that being far without the Orb of the Earth, and in itself a very small Body, it appeared only like a little Speck of a Cloud, such as would scarce have been discovered in an ordinary Telescope, much less by the naked Eye."

The Markings on Jupiter.

GENTLEMEN,—

In your publication for June there is an interesting article by Mr. Green, and some notes by Mr. Barnard, relative to the markings and spots on Jupiter in March and April this year. Whilst engaged on the satellites rather closely of late, I have unavoidably noticed the appearance of Jupiter itself, and as so many changes began to occur in February, I commenced to note the principal ones carefully. I may here perhaps be permitted to state one point. I cannot help fancying these phenomena gather themselves into groups, of no definite duration as to endurance, and not separated by any definite period of rest. I mean, at least so it occurs to me, Jupiter remains in a quiet inactive state for some time, then suddenly awakens into activity, exhibiting almost every rotation some small variation in appearance, and then sinks into a quiet inactive comparatively passive state. I understand my ideas are not new, for Ranyard expressed almost as much as far back as 1870, but of this I have only lately been aware. For some time it has been urged by certain observers that Sun-spots were more or less in activity according to the position of Jupiter in its orbit, but Ranyard goes even further and by his arguments presses upon one the belief that an outbreak on the Sun is frequently, if not always, coincident with a similar period of activity on Jupiter: not that he thinks they are in any way related as cause and effect, but merely that they are dependent upon some extraneous cosmical change. As far as my own observations go, March was, take it all in all, more prolific in changes than April, and, strange to say, March was more prolific also as regards the Sun-spots. Through the kindness of Mr. Whipple of Kew Observatory, I append in the footnote a brief record of these phenomena in March and April *.

Relative to the black lines observed by Mr. Green, I confess not to have seen them until March 10—at least I find no previous note; but on that night I have a record to the effect that a peculiar black line was seen 10.30 P.M., and in the same position as described by him. Both of us apparently availed ourselves of the beautiful defining-power of the night of March 15, when I read "the black

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* Mar.	S.-spots.	Mar.	S.-spots.	Apr.	S.-spots.	Apr.	S.-spots.
2	3	19	1	1	2	18	2
4	4	20	1	2	3	20	2
5	4	21	1	4	3	21	2
7	4	24	1	7	3	22	3
9	3	25	1	13	2	23	3
12	3	26	2	14	2	25	3
13	3	27	2	15	2	27	4
16	2	28	2	16	2	28	4
17	1	30	1	17	2	30	4
18	1	31	1				

Note.—The spots underlined were both numerous and large on March 4, 5, 7.

line is followed by another," and a query as to whether the second is new, or formed by a separation into two of the single one seen March 10. They were curious lines and attracted my attention greatly—not so much perhaps as regards their size, although they were certainly large, but more especially as to their defined appearance. They looked as if drawn with ruler and pen. I was doubtful whether each line was uniformly black throughout its entire length, but by using my occulting eyepiece and hiding the greater portion of the planet, I found no apparent difference. I was unable from bad weather and other interruptions to re-examine Jupiter until the night of April 3, when one line had rotated out of sight I thought, and the second was only partly visible. Mr. Green noticed a third and smaller line (shorter, I believe, than the others), but this I failed to see; perhaps his 18-inch would show this faint marking, which mine (8 inches less in aperture) would not. Whether these lines disappeared before Mr. Barnard's records were made, or whether they happened to have escaped his notice from being on the other side of the planet when he was observing, would be interesting to know, for he certainly mentions no lines, only several spots white or black; but it would be more interesting still if he or others could furnish any observations which might lead to our knowing whether these black lines degenerate into black spots.

As to *white* marks or spots, there certainly have been a great number, and to marshal them into suitable tabulation, as regards position and size, would be a task of considerable difficulty, for I cannot help thinking they have a proper motion of their own—an old idea, I allow, but still one which even yet has not been definitely proved, I think. I may mention, however, two peculiar ones of exceeding brightness and lustre, with well-shaded bases, which were on the meridian upon the planet's equator at 10.30 P.M., Feb. 17 *. These were augmented by a third following them, when I next examined the planet March 1. Another spot (in a position to me very unusual) was seen April 4: it was midway between the south belt and the south pole, nearer the pole perhaps, and was peculiar both in size and in having such a decidedly shaded base. This shading was more on one side than the other (unfortunately my note does not say *which* side) and gave rise to the thought that Mr. Brett's view—that these spots cast a shadow—is correct. Mr. Barnard noticed a spot almost precisely in the same position (it was a little nearer the equator) three weeks after my observation, but it was "dark." It would be interesting to know if these white spots turn black after a while, and more so still if it could be shown they drifted towards the equatoreal regions. As far as I can gather, the position is very unusual; the first chronicled, I think, was by Lassell about 1858.

In conclusion, it will be worthy of careful observation to see how many of these markings reappear next year, and, as Mr. Green states, possibly those that survive the coming conjunction *may* throw light on the formation of even the old red spot itself.

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* Since writing this, Mr. Green has exhibited at the last meeting of the Society some exquisite drawings of these very spots.

Ivy House, Clapham Common, June. EDMUND J. SPITTA.

Large versus *Small Telescopes.*

GENTLEMEN,—

In the March number of the 'Observatory,' Mr. Denning discusses Jovian phenomena as seen with large and small telescopes, and in subsequent numbers the subject has been discussed by others.

During the past twenty-five years I have used refractors of various sizes, ranging from 4-inch up to 18½-inch object-glass, and have no hesitation in saying that I can see more and better under *any* atmospheric conditions with the 18½-inch than with any smaller telescope.

To know whether the seeing is good or bad, it is not necessary to use a large aperture, an object-glass of 6 inches is just as good for that purpose as one of larger size. When the seeing is good for 6 inches it will be good for 18 inches. Whenever meridian observations are made with the 6-inch object-glass, the condition of the images of stars gives one a pretty good idea of what the seeing will be with the 18½-inch equatoreal.

Mr. Green states that on one occasion Saturn was well seen with a 5-inch refractor, when it could not be observed at all with a 12-inch reflector.

I am not familiar with reflecting telescopes, but I believe it is generally conceded that the figure of the mirror is liable to distortion from temperature or other causes, a fault not common to refractors. I think if he found excellent seeing with a refractor of 5 inches, he would have found it equally good with 20 inches.

The 5th and 6th stars in the Trapezium of Orion were formerly supposed to be variable, because sometimes they could not be seen. Now a telescope of 3- or 4-inch aperture may show them when the seeing is exceptionally good, while a large object-glass will effect the same when the seeing is too poor for any useful work.

In my experience a large telescope will always do the work of a smaller one, and generally do it better. But to utilize the full power of a large object-glass is another question.

In the climate of the United States the nights on which the highest powers may be used are rare. I presume three fourths of all the observations by Mr. Burnham and myself, with the Chicago refractor, were made with a magnifying-power of 300 and the balance with higher powers up to 925. But even on indifferent nights, when 300 is the best working-power, many double stars may be measured, which, under equal atmospheric conditions, would be impossible with a smaller aperture.

Again, on an exceptionally good night, when the telescope will bear a power of 900, one may make observations entirely beyond the reach of smaller instruments.

I do not entirely agree with Prof. Hall, with regard to the statement that a large telescope does not show enough of detail in planetary markings. On the contrary, I believe it will show the markings better than a small one. This is not a question of opinion, however, but may be put to a practical test by reducing the aperture, or, as Prof. Young has done, by using a large and small telescope on the same object.

I have frequently tried reducing the aperture of the Chicago refractor on Jupiter observations, but never found any advantage, everything was simply fainter.

I do not understand why the increased illumination of a planet's disk by the use of a large aperture is objectionable to seeing, since every part is proportionally illuminated. One can certainly see objects better in a brilliantly illuminated apartment than in one indifferently lighted.

Mr. Denning has taken exception to my statement regarding the red spot on Jupiter. When that criticism was written I had not seen Mr. Denning's original article; but having since read it, I still affirm the correctness of my views.

I have no doubt that, with a small telescope, I should have seen just what he and others saw, as the spot was apparently against a faint belt following, but at no time was it "merged into the belt."

From his citations it appears that the red spot was merged into the belt from January 24 to February 25, 1884. I find observations were made at the Dearborn Observatory, for longitude of red spot, on January 17, February 9, 22, March 9, &c.

On February 9 its latitude was determined, and on February 22 and March 9 both latitude and length. At the latter date its colour was stated as pale pink. I certainly should not have measured the length of the object unless the outline was sharp and well defined.

But the fact that subsequently it was seen by everybody separate and distinct, leads us to infer that it did not coalesce at any time.

It is stated in text-books on Astronomy that new belts may be formed on the planet in the course of an hour or two, and the idea is conveyed that the changes taking place on the surface of the planet are so rapid that in a few days we may have an entirely different set of phenomena. I object to this view of the subject as being based on insufficent data and a misinterpretation of observations.

I do not deny that minor changes occur from day to day, but, on the other hand, there are many salient features which are maintained for years with but slight variation—the great red spot is a good illustration. This object has never been lost sight of, with the Chicago telescope, since 1879 to the present time, whenever the planet was far enough from the Sun for observation. Its shape and size have been retained in a remarkable manner. During 1883–84 it was quite faint, but during the past opposition it has been more conspicuous

In the May number of this journal Prof. Young has called attention to the ring-like form of the spot as seen by him in March last. This phenomenon was first noticed here on the 29th February, when the ring was very well defined, the colour being similar to that of the great equatoreal belt, while the centre of the spot was of the general colour of the disk. The nearest preceding observation was made on February 2, when the following notes were recorded:—Seeing fair; outline of spot sharply defined; shape same as formerly; paler than the equatoreal belt, but reddish in colour. It would appear from these observations that the ring was formed during the month of February.

(The weather during the past winter was excessively cold and stormy, and the nights on which observations could be made were rare.)

The ring-shape of the spot continued without material change for a month or more, then it began to gradually close up, and on May 15 there was only a small strip of white over the middle. The observations during the early part of June showed the spot to be apparently all of one colour—a pale pink; but the planet was not favourably situated for critical observation.

An elliptical ring, very similar to the one described and occupying the same position in latitude, was observed in 1869–70; but seems to have disappeared, as we have no record of a spot in this locality until 1878, when the present red spot was detected. It is possible, however, that the present spot is identical with that of 1869, as has been suggested by Mr. Denning.

The cause of change in structure of this object is difficult to explain. If, however, we assume the spot to be a hollow globular mass, and it should be covered with clouds, the colour would remain most intense at its outer margin, because at that point we would look through the greatest thickness of matter, and hence it would appear as a ring. As the covering of clouds became thinner, the ring would gradually close up, and the same colour would be seen over its whole surface.

Dearborn Observatory, Chicago, U. S.,
1885, June 24.
G. W. HOUGH.

Photographing the Solar Corona by a New Method.

GENTLEMEN,—

In photographing an object of such intense brilliancy as the Sun, a formidable drawback to some researches is found in the diffuse reflection from scratches or dust on the speculum or object-glass employed. When photographing Sun-spots or other well-defined markings a little false detail is not of very much consequence; but when our object is to separate the corona from the atmospheric glare, false detail cannot be permitted at all. I believe an efficient remedy for this evil is to use "nature's own camera"—a minute hole in a screen.

I have this method under trial, and will shortly commence daily photographs of the corona. These will be of interest in connection with those about to be made at the Cape of Good Hope by Mr. C. Ray Woods.

T. S. H. SHEARMEN.

Brantford, Canada,
1885, June 11.

NOTES.

ROYAL OBSERVATORY, GREENWICH.—The annual visitation of this Observatory took place on Saturday, June 6, when the Astronomer Royal presented his Report to the Board of Visitors. Under the head of Astronomical Observations considerable prominence is given to the employment of a reversion prism in observations with the transit-circle and its collimators and with the altazimuth, in order to detect personality due to direction, if such exist, in measurements or observations of transits. The collimation observations show no sensible personality of this kind, but in the transit observations sensible differences between the reversed and ordinary observations of clock-stars are found for some observers—probably, the Astronomer Royal thinks, those who have not yet settled down into a definite habit of observing stars which appear to move in the reverse direction. In order to determine the observers' personal equations in the observation of slow-moving as well as of quick-moving stars, and of limbs of the Sun, Moon, or planets, the Astronomer Royal has arranged in concert with Mr. Simms a Personal Equation Instrument to be used with the transit-circle. In this instrument, which is not yet completed, an artificial sun or star placed in the focus of an object-glass of 7 inches aperture and 50 feet focal length, which is attached to the dew-cap of the transit instrument, is carried smoothly from east to west, or from west to east, at a rate which can be varied at will from that of a very close circumpolar star to three or four times that of an equatoreal star. The times of the transits of the artificial sun or star over the wires of the transit-circle are not only registered by the observer in the usual way, but also recorded themselves automatically on the chronograph. Since last October, transits of the close circumpolar stars have been taken at the middle wire set to successive revolutions of the R. A. micrometer. The equality of the successive intervals of this micrometer has been carefully tested. The subjects of meridian observations have been, as usual, the Sun, Moon, planets, and fundamental stars with other stars from a working catalogue which now contains about 2750 stars, about 380 stars having been lately added to the list from the 'Harvard Photometry' with a view of making the forthcoming Greenwich catalogue of stars down to the sixth magnitude as complete as possible. The annual catalogue of stars observed in 1884 contains about 1370 stars. In the twelve months ending 1885, May 20, 5523 transits and 5321 zenith distances were observed with the transit-circle.

Under the heading Clocks and Chronograph the Astronomer

Royal states that "on January 1 the public clock at the Observatory entrance and the other mean solar clocks were put forward 12 hours, so as to show Greenwich civil time, starting at midnight and reckoning from 0^h to 24^h, which would correspond with the universal time recommended by the Washington Conference. The change from astronomical to civil reckoning has also been made in all the internal work of the Observatory, but its introduction into the printed astronomical observations has been deferred to allow time for a general agreement amongst astronomers to be arrived at. It is proposed, however, to adopt the civil day without further delay in the printed magnetical results." The equatoreals are all in good order with the exception of the Lassell, in which, though difficulties arising from insufficient stability of its base, and the bad driving of the clock, have been successfully surmounted, trouble "is still experienced from want of stability of the optical axis of the large mirror, which requires to be readjusted continually as the telescope is moved."

In the spectroscopic department, the observations for the determination of the motions of stars in the line of sight have been much more numerous than usual, 641 measures having been obtained of the displacement of lines in various stellar spectra, "besides measures of the displacements of the *b* and F lines in the spectra of the east and west limbs of Jupiter, and of the east and west ansæ of the rings of Saturn, and comparisons with lines in the spectrum of the Moon, or of the sky, made in the course of each night's observations of star-motions, or on the following morning, as a check on the general accuracy of the results for star-motions. The observations of the last twelve months confirm the change in the motion of Sirius, which now appears to be approaching the Sun at the rate of about 20 miles a second. As there is great difficulty in the use of a pointer or cross-wires for measuring both the broad dark line in the star's spectrum and the narrow bright comparison line, Mr. Maunder has suggested the use of a Reversion Spectroscope (on the double-image principle) for these observations, and Professor Pritchard has kindly lent the reversion spectroscope of the Oxford University Observatory, in order that the suitability of that form of instrument may be tested."

The photographs of the Sun show that the mean spotted area of the Sun was slightly less in 1884 than in 1883, and slightly greater than in 1882, whilst the faculæ in 1884 showed a slight increase as compared with 1883, and a slight falling off as compared with 1882. It would seem that the maximum both of Sun-spots and faculæ occurred about the end of 1883 or beginning of 1884. There have been only two days since the date of the last report on which the Sun's disk was observed to be free from spots. The magnetic registers give evidence of five days of great magnetic disturbance in 1884, but there were also about 20 days of lesser disturbance for which tracings of the registers will be published. The new self-registering thermometer apparatus has been provisionally mounted in the south ground in order that a series of daily records may be obtained for comparison with the existing apparatus before the latter is removed. The mean temperature for 1884 was 50°·7, being 1°·4 higher than the average of the last 43 years. The maximum temperature in the shade was 94°·1 on Aug. 11; the minimum 24°·5 on Nov. 25. The greatest mean and least daily motion of the air were 891, 286, and 78 miles, the first on Jan. 23, the last on Feb. 8. The number of hours of bright sunshine recorded in 1884 was 1115, or about 100 hours less than the average of the seven preceding years. The rainfall was 18·0 inches, or about 7 inches below the average of the last 40 years.

In connection with the establishment of hourly time-signals at the Start or Lizard, the Astronomer Royal states that he has received from the Committee of Lloyds an assurance that that corporation would be willing to undertake the maintenance of hourly time-signals at any of their signal stations, provided the Government would supply the necessary apparatus. After consultation with Captain Wharton, it has been thought better that, before taking further steps, some preliminary trials should be made of a collapsible cone as an hourly time-signal, facilities of doing which exist at Devonport. The Astronomer Royal proposes that the time-signal should be made automatically by a local clock to be corrected daily by a time-signal from Greenwich at 10 A.M., which should start an auxiliary seconds' pendulum suspended freely just behind the clock pendulum.

Under the head of Personal Establishment we are informed that Mr. Dunkin "retired on Aug. 25, after an honourable service of forty-six years, which has been throughout characterized by remarkable zeal and ability, and has contributed largely to maintain the credit of the Observatory. Mr. Dunkin has been succeeded in the post of Chief Assistant by Mr. H. H. Turner, B.A., of Trinity College, Cambridge. The only other change to notice is that, consequent on a recommendation of the Board of Visitors at their last meeting, the Admiralty have raised the Spectroscopic and Photographic Assistant to the position of a First-class Assistant in recognition of his increased duties and responsibility."

The Report concludes with urging the necessity that the Observatory should possess a refractor of a much greater aperture than that of the S.E. equatoreal (12¾ inches), which is too small to allow of any but the brightest stars being observed successfully with the spectroscope, and it is proposed that an object-glass of 28 inches aperture and of 28 feet focal length should be carried by the present mounting of the S.E. equatoreal in the place of the much smaller instrument which it now supports.

THE PARIS OBSERVATORY.—We have received Admiral Mouchez's report on the state of the Paris Observatory for the year 1884. In addition to the usual annual report, M. Mouchez has issued a special report on the subject of the branch Observatory outside Paris, the establishment of which he had advocated, and the necessary cost of which he had proposed to meet by the sale of some of the ground surrounding the Observatory. This plan was, however, rejected by the Academy of Sciences, and in consequence of this the prospects of the Observatory, in M. Mouchez's opinion, are indeed gloomy. It is found impossible to finish and to mount the great telescope of 16^{m} focal length, commenced 30 years ago—in fact the employment of instruments of great precision and of great optical power is not possible, under favourable conditions, at the Paris Observatory. The instability of the ground militates against the effective use of the former, and the want of transparency of the Parisian atmosphere against that of the latter. It is manifestly quite useless to erect large and powerful instruments in an Observatory surrounded on all sides by buildings. Inferior instruments will do all that it is possible to do under such conditions. It is also impossible, as long as the Observatory remains on its present site, to introduce the much needed reform of supplying house-room for the staff inside the walls. Without such provision it is difficult to obtain a sufficiently continuous attendance during the evening, and absolutely impossible to arrange for uninterrupted work throughout the 24 hours. It is hoped that the Council of the Observatory will consider how it may be possible to overcome some, at all events, of the difficulties which M. Mouchez has pointed out.

Turning now to the report on the work done during last year, we find that the revision of Lalande's Catalogue having been almost completed, two only of the meridian instruments have been employed for these observations, and a third has been devoted, at the request of M. Loewy, to the determination of positions of circumpolar stars. Certain improvements have been carried out in the latter instrument (Cercle du Jardin), but M. Mouchez points out that he has no meridian instrument with which he can undertake observations of fundamental stars, and is in doubt as to what class of work he should take up when Lalande's Catalogue shall have been completed. Altogether 18,014 meridian observations were made during 1884. Of these 996 were observations of planets or comets, including 155 of the Sun and 121 of the Moon. With regard to work carried on with the equatoreals during the year, it is pointed out that in consequence of the rejection of M. Mouchez's plan, mentioned above, it is impossible to make use of the great telescope of $0^{m}\cdot74$ aperture; a proposal has in fact been made that it should be transferred to the Algiers Observatory, where the climatic conditions are so exceptionally favourable. The equatoreal of the West Tower has been used by M. Bigourdan for observations of double stars, minor planets, comets, and nebulæ, 15 new nebulæ having been discovered during the year. M. Bigourdan found that it was possible to observe, for some time of the evening, on 223 nights; on 95 of these it was clear for 5 hours or longer. With the equatoreal of the East Tower MM. Henry have continued their observations of the satellites of the large planets, and have studied the physical aspect of Jupiter, Saturn, Uranus, and Neptune. The *équatorial coudé* has been employed to observe planets and comets recently discovered. A series of photometrical observations of the satellites of Jupiter have also been carried out by M. Obrecht with this instrument. The important contribution to astronomical photography recently effected by MM. Henry has been already noted in 'The Observatory' (Vol. viii. p. 219); a reproduction of their photograph of the Cluster in Perseus is appended to this report. The reduction as well as the printing of the Lalande Catalogue and the other astronomical works appear to be in a forward state. We regret to notice, however, that M. Mouchez finds that the cost of publication of the 'Bulletin Astronomique' is a severe strain on the resources of the Observatory, and expresses his fear that unless the number of subscribers is considerably increased, it will be impossible to continue it. It would be indeed a misfortune for our science, not only in France, but throughout the civilized world, should this valuable periodical cease to appear. We fear, however, that, from the nature of the case, it could hardly be expected to be self-supporting.

Notwithstanding the many difficulties in his way, as M. Mouchez has clearly and frankly explained, we venture to think that he is to be congratulated on the amount and quality of the work turned out by the astronomers attached to the Paris Observatory, and to express a hope that in the near future they may be in a position to utilize the larger instruments referred to in this report, which at present are lying idle.

MARS IN 1884.—The planet Mars, always a somewhat difficult object for the draughtsman, is seen under such disadvantageous circumstances when his northern hemisphere is presented to us that the regions round the north pole are far less well known to us than the southern and equatoreal portions. We owe, therefore, a special debt of gratitude to those astronomers who are sufficiently earnest and painstaking to study the planet at the less inviting oppositions; and this gratitude Mr. Knobel has now twice earned, having presented us with a beautiful series of drawings made in 1873 ('Monthly Notices,' Vol. xxxiii. No. 8) and now again with another, not less excellent, taken during the last opposition, and which has just been published in the 48th volume of the 'Memoirs of the Royal Astronomical Society.' These latter drawings are in very close accordance with the earlier, and establish clearly the permanence of the principal markings—an important matter, since the dark spots in the northern hemisphere lack the sharp outlines and characteristic forms which distinguish the principal spots of the southern hemisphere, and which render their observation so much more attractive and easy. On the whole Mr. Green's chart (Mem. R. A. S., Vol. xliv.) is confirmed, but a few points seem to require modification. Dark bands were always seen to the west of "Knobel Sea" (Mr. Green's nomenclature), and "Leverrier Land" could never be detected. Prof. Schiaparelli's elaborate series of "canals" was not identified. The "Læstrygonum" and "Cyclopum" were indeed seen; but these have been frequently observed, the latter being the well-known "Huggins Inlet" of Proctor's chart. But the district east of the "Cyclopum" on one occasion showed a cobweb-like mottling, that without showing the straight, hard parallel lines of Schiaparelli's 1881 chart, yet recalled something of its *general* appearance. It is interesting to note that "Phillips Island" again appeared united to "Beer Continent," as in the drawings of Mädler in 1830, Lockyer in 1862, Schiaparelli in 1877, and others, instead of being separated from it by a prolongation of "Herschel II. Strait," as in Dawes's drawings in 1864, and Green's in 1877.

Mr. Knobel suggests two reforms in Martial cartography. One, the transference of the prime meridian from "Dawes Forked Bay" to the "Oculus" or "Terby Sea," would be a decided improvement, since the latter is the most conspicuous, definite, and stable marking of the same size on the planet, and being small and nearly circular, is admirably adapted to serve as a fiducial point. If not taken as the actual zero, at least longitude 90° should pass through its centre. The other the substitution of lettered regions for the names introduced by Proctor, Green, and Schiaparelli would, we think, be a mistake. There is a considerable difference in the proportions given to certain regions by different astronomers who yet agree as to their general form (*cf.* Green's "Mädler Continent" with Schiaparelli's "Chryse"), and this would often render uncertain the identification of the markings referred to. Besides there is a decided advantage in having a system which indicates whether the spots referred to are bright or dark; for this is what is practically implied by the terms "seas," "bays," "islands," and "continents." Such names also often give a rough indication of the shape or size of the object referred to, which a "K" or "ϕ" would not do. At present, confused though Martial nomenclature is, it seems the best way to use either Green's and Schiaparelli's, falling back on the second when the first fails us; for as yet no other charts comparable to theirs in accuracy or multiplicity of detail have appeared. Such a combination will not be one whit more bizarre than the names actually in use without sensible inconvenience for lunar markings. The astronomer who in the years to come can surpass their drawings, may fairly be called upon by his contemporaries to prepare a more perfect chart, and to devise a more acceptable nomenclature.

THE PROPOSED CHANGE IN THE ASTRONOMICAL DATE.—Mr. W. C. Winlock, in the number of 'Science' for June 26, 1885, gives a short abstract of Profs. Struvé's and Oppolzer's recently expressed opinions on this subject. Mr. Winlock concludes his paper with the following remarks:—

"It is difficult to see how this matter will finally be decided. It is evidently a question for astronomers to settle among themselves but so far they seem to be very evenly divided. For instance: out of some twenty-seven astronomers whose opinions, more or less decided, have been accessible for a count, thirteen seem inclined to favour the proposed change, while fourteen are opposed to it. And among the *pros* are Adams, Struvé, and Christie; among the *cons*, Newcomb, Foerster, and Auwers."

TUTTLE'S COMET.—This comet returns to perihelion this year under circumstances which are not favourable to its re-observation without a good ephemeris. For the last return in 1871, the perturbations during the previous return were very accurately determined by Tischler, who lost his life before Metz in the Franco-German war; and the comet was detected at Marseilles some seven weeks before perihelion passage.

The following elements and ephemeris are by Dr. Johannes Rahts:—

Perihelion passage, 1885, Sept. 11·1799

Longitude of perihelion..........	116° 28′ 58″·79	Mean Equinox 1890.
,, ascending node......	269 42 1 ·46	
Inclination	54 19 45 ·45	
Eccentricity	55 14 22 ·58	
Log. semi-axis major	0·7590765	

	R.A.			Decl.				
	h	m	s	°	′	″	Log Δ.	I.
Aug. 1....	6	56	4·61	+32	37	50·9	0·29477	0·36
3....	7	3	1·72	31	32	17·4	0·29102	
5....		9	54·56	30	24	19·5	0·28729	0·39
7....		16	43·28	29	13	57·8	0·28358	
9....		23	28·05	28	1	13·0	0·27991	0·42
11....		30	9·04	26	46	6·2	0·27629	
13....		36	46·47	25	28	39·0	0·27272	0·46
15....		43	20·56	24	8	53·1	0·26921	
17....		49	51·57	22	46	50·5	0·26577	0·49
19....	7	56	19·80	21	22	33·9	0·26241	
21....	8	2	45·53	19	56	6·4	0·25915	0·52
23....		9	9·09	18	27	31·4	0·25599	
25....		15	30·79	16	56	53·2	0·25294	0·54
27....		21	50·97	15	24	16·6	0·25003	
29....		28	9·97	13	49	47·0	0·24725	0·58
31....	8	34	28·13	12	13	32·0	0·24463	

The brightness on Oct. 12, 1871, is taken as unity.

THE observations made by Dr. Dunér and Prof. Thiele in 1879, to determine the difference of longitude between Lund and Copenhagen, have been reduced by the latter. The Lund Observatory is $2^{m}\ 26^{s}{\cdot}38 \pm 0^{s}{\cdot}04$ east of the Observatory of Copenhagen.

In the Report of the Harvard College Observatory for 1884 Prof. Pickering states that, in consequence of the diminished resources of the Observatory, the observations of star-spectra, &c. would most probably be left incomplete. In our last issue we expressed the hope that this misfortune might be averted; and it is with great pleasure we note the munificent bequest of nearly $300,000 from the will of the late Mr. R. T. Paine, of Brookline, Mass. The observatory receives one half of this amount at once, the remainder at the death of the widow. Mr. Paine took great interest in astronomy, and was very zealous in observing eclipses, occultations, &c. His meteorological record is said to be continuous for the past 50 years.

The degree of LL.D. has been conferred on Benjamin Apthorp Gould by Harvard College.

We regret to announce the decease, on May 25, of Prof. Th. Clausen, Foreign Associate R.A.S., Emeritus Director of the Dorpat Observatory.

The Minor Planet No. 248 has been named Lameia.

Elements and Ephemeris of Comet 1885 a (Barnard).

By Dr. T. Holetschek *.

A comet was discovered by Mr. E. E. Barnard, Nashville, Tennessee, on July 7. The comet was faint, about the 11th magnitude, with some central condensation, but no tail.

Elements.

T = 1885 Sept. 25·1565 Berlin M.T.

$$\begin{array}{ll} \pi - \Omega \;\ldots\ldots & 198^\circ\ 42'\ 31'' \\ \Omega \;\ldots\ldots\ldots & 93\ \ 30\ \ 23 \\ i \;\ldots\ldots\ldots\ldots & 75\ \ 52\ \ 57 \end{array} \left.\vphantom{\begin{array}{l} a \\ b \\ c \end{array}}\right\} \text{Mean Eq. 1885·0.}$$

$\log q$ ...... 0·36083

Error of middle place (O − C).

$$d\lambda \cos \beta = -3''$$
$$d\beta = +2$$

Ephemeris for Berlin Midnight.

		α.		*δ.*				Brightness.
1885.	h	m	s	°	′	Log Δ.	Log *r*.	
July 15..	17	7	20	− 9	17·3	0·1926	0·3863	1·00
19..	17	0	33	11	33·1	0·1981	0·3837	0·98
23..	16	54	16	13	46·2	0·2051	0·3812	0·96
27..	16	48	32	15	55·8	0·2136	0·3788	0·94
31..	16	43	23	18	1·1	0·2232	0·3766	0·91
Aug. 4..	16	38	41	−20	1·5	0·2337	0·3745	0·87

The brightness on July 9 is taken as unity.

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* Circular der k. Akad. der Wissen. in Wien, No. liv.

Maxima and Minima of Variable Stars in September 1885.

M, signifies maximum; *m*, minimum.

Sept. 1 R Sagittæ, *m*.
1 *o* Ceti, *m*.
2·6 ζ Geminorum, M.
3 S Boötis, M.
3 R Sculptoris, M.
4 S Herculis, *m*.
4 R Ophiuchi, M.
6 R Hydræ, M.
7 R Draconis, *m*.
7 R Virginis, *m*.
7·7 ζ Geminorum, *m*.
8 V Virginis, M.
8·3 T Monocerotis, M.
Sept. 9·6 W Sagittarii, *m*.
12·7 ζ Geminorum, M.
13 R Crateris, *m*.
16 U Monocerotis, M.
16 R Coronæ, *m*.
18 S Vulpeculæ, M.
22 V Cygni, M.
24 V Tauri, M.
25 R Lyræ, *m*.
27 T Serpentis, M.
27·4 T Monocerotis, *m*.
29 U Virginis, M.
30 R Leporis, M.

Variables of Short Period.

δ Cephei.	d.
Sept.	6·35, M.
	11·65, M.
	15·45, *m*.
	22·45, M.

U Sagittarii.	d.
Sept.	1·6, *m*.
	4·5, M.
	8·3, *m*.
	11·3, M.
	28·5, *m*.

η Aquilæ.	d.
Sept.	4·5, M.
	11·7, M.
	16·5, *m*.
	23·7, *m*.

U Cephei, *m*.		h	m
Sept.	2	9	27
	7	9	6
	12	8	46
	17	8	25
	22	8	5
	27	7	44

Algol, m.		h	m
Sept.	12	7	20
	26	15	24
	29	12	13

λ Tauri, *m*.		h	m
Sept.	1	10	6
	5	8	59
	9	7	51
	13	6	43

S Cancri, *m*.		h	m
Sept.	17	8	12

δ Libræ, *m*.		h	m
Sept.	1	14	1
	8	13	35
	15	13	9
	22	12	43
	29	12	17

U Coronæ, *m*.		h	m
Sept.	3	8	57
	13	17	30
	20	15	13
	27	12	55

Astronomical Memoranda, September 1885.

Equation of Time:—Sun *before* Clock, Sept. 1, 0^{m} 14^{s}; Sept. 11 3^{m} 34^{s}; Sept. 21, 7^{m} 5^{s}; Sept. 30, 10^{m} 8^{s}.

Sidereal Time at Mean Noon:—Sept. 1, 10^{h} 43^{m} 15^{s}; Sept. 11 11^{h} 22^{m} 41^{s}; Sept. 21, 12^{h} 2^{m} 6^{s}; Sept. 30, 12^{h} 37^{m} 35^{s}.

Sun.		Rises.		Sets.		Position-angle of axis.		Heliogr. co-ordinates of centre of disk. Lat.		Long.	
		h	m	h	m	°	′	°	′	°	′
Sept.	3	17	19	6	42	21	57	+7	14	341	59
	8	17	27	6	30	23	5	7	15	275	57
	13	17	35	6	19	24	3	7	12	209	56
	18	17	42	6	7	24	53	7	6	143	56
	23	17	49	5	56	25	32	6	57	77	56
	28	17	58	5	46	26	2	+6	45	11	56

The Moon.

	rises.			*sets.*			*sets.*			*rises.*	
	h	m		h	m		h	m		h	m
Sept. 1	10	9	Sept. 9	6	44	Sept. 17	11	33	Sept. 25	6	31
2	10	56	10	7	11	18	12	29	26	6	59
3	11	52	11	7	40	19	13	27	27	7	32
4	12	58	12	8	10	20	14	28	28	8	9
5	14	11	13	8	42	21	15	33	29	8	53
6	15	27	14	9	17	22	16	39	30	9	46
7	*sets*		15	9	58	23	*rises*				
8	6	15	16	10	43	24	6	5			

Sept. 1^{d} 17^{h} 15^{m} Last Quarter.
15 18 15 First Quarter.
30 23 29 Last Quarter.

Sept. 8^{d} 8^{h} 43^{m} New Moon.
23 19 55 Full Moon.

Planets for September 1885.

Day.	Planet.	R.A.			Dec.		Diam.	Rises.		Tr.		Sets.	
		h	m	s	°	′	″	h	m	h	m	h	m
1	Mercury..	10	46	1	3	26 N.	10·6	17	29	0	3	6	25
11		10	24	55	8	33 N.	8·8	16	17	22	59	5	50
21		10	52	32	8	31 N.	6·6	16	11	22	51	5	37
30		11	45	53	3	36 N.	5·4	16	53	23	10	5	30
1	Venus ..	12	40	42	3	47 S.	12·3	20	16	1	57	7	42
11		13	24	46	8	50 S.	12·9	20	47	2	2	7	21
21		14	9	43	13	36 S.	13·5	21	19	2	7	7	0
30		14	51	22	17	28 S.	14·2	21	46	2	14	6	46
1	Mars	7	28	45	22	39 N.	5·5	12	34	20	44	4	57
11		7	55	32	21	43 N.	5·7	12	27	20	31	4	38
21		8	21	23	20	33 N.	5·9	12	22	20	18	4	16
30		8	43	48	19	20 N.	6·1	12	17	20	5	3	55
1	Saturn ..	6	27	12	22	24 N.	15·8	11	32	19	41	3	53
30		6	34	44	22	19 N.	16·7	9	46	17	54	2	5
1	Neptune..	3	34	46	17	28 N.		9	11	16	48	0	29
30		3	33	39	17	23 N.		7	17	14	53	22	29

Sept. 2^{d} 6^{h}. Mercury in inferior conjunction.
10 21 Mercury stationary.
18 7 Mercury at greatest elongation (17° 51′ W.).
25 19 Uranus in conjunction with the Sun.

A Total Eclipse of the Sun September 8, invisible at Greenwich.

		Long.	Lat.
Begins on the Earth generally	8^{d} 6^{h} 36^{m}·1 in	171° 53′ E.;	16° 16′ S.
Central Eclipse begins ,,	7 56 ·5 in	154 54 E.;	41 0 S.
,, ,, at Noon	9 19 ·9 in	140 39 W.;	57 46 S.
,, ,, ends generally	9 47 ·6 in	77 38 W.;	74 43 S.
Ends on the Earth generally	11 7 ·9 in	84 13 W.;	50 38 S.

A Partial Eclipse of the Moon September 23, partly visible at Greenwich.

First contact with penumbra 23^{d} 17^{h} $2^{\mathrm{m}}{\cdot}1$; with shadow 18^{h} $15^{\mathrm{m}}{\cdot}0$; middle of eclipse 19^{h} $48^{\mathrm{m}}{\cdot}3$; last contact with shadow 21^{h} $21^{\mathrm{m}}{\cdot}6$; with penumbra, 22^{h} $34^{\mathrm{m}}{\cdot}5$. Magnitude of the eclipse (Moon's diameter=1) 0·784.

The first contact with the shadow takes place at 37° E. of N. point of Moon's limb; the last contact at 73° W. of N. point; for *direct* image.

Occultations for September 1885.

Day.	Star.	Mag.	Disap.	Angle.	Reap.	Angle.
			h m	°	h m	°
1	θ^2 Tauri	4½	10 1*a*	97	10 52	285
1	θ^1 Tauri	4½	10 2*a*	117	10 51	265
1	B.A.C. 1391	5	11 1	155	11 32	228
1	85 Tauri........	6	11 21	59	12 1	325
2	117 Tauri	6	10 34*a*	117	11 20	257
2	B.A.C. 1728	6	12 13	48	12 48	328
19	13 Capricorni	6	8 4	127	9 23	264
19	14 Capricorni	5	9 33	141	10 41	253
20	18 Aquarii	6	6 47	73	7 55	319
21	B.A.C. 7774	6	10 8	136	11 22	270
23	B.A.C. 8365	6½	17 12	86	18 5*a*	311
25	μ Piscium	5	8 12	132	9 9	269
26	B.A.C. 741......	6½	9 19	65	10 3	335
28	48 Tauri........	6	9 56	73	10 45	314
28	γ Tauri	4	11 46	74	12 43	314
28	75 Tauri........	6	16 41	136	17 41	242
28	θ^1 Tauri	4½	16 48	39	17 26	338
28	B.A.C. 1391	5	17 41	84	18 51	289
29	111 Tauri	5½	15 2	98	16 17	275
29	117 Tauri	6	17 15	45	18 5	321

a. Star below the horizon.

On Sept. 1, 13^{h} 40^{m}, Aldebaran approaches very close to Moon's limb (193°).

The R.A. and N.P.D. of Planets and the Solar Co-ordinates are given for Greenwich Mean Noon; in all cases Greenwich Mean Time is used, reckoning from noon to noon, as in previous years; the angles for Occultations are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

ERRATUM IN No. 98.

Page 209, line 3, *for* refractor *read* reflector.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 101. SEPTEMBER. 1885.

Annual Meeting of the Liverpool Astronomical Society.

The fourth Annual General Meeting of this Society was held on Saturday, July 25, Mr. R. C. Johnson, Vice-President, in the Chair. The report showed that the Society numbered 258 members as against 166 last year. The total income of the Society had been £72 16*s*. 6*d*. and the expenditure £75 4*s*. 6*d*. There was also an outstanding account for printing of £23.

Mr. Isaac Roberts, F.R.A.S., said that this would leave a total deficit of more than £25. He thought the amount for printing was larger than they were warranted in incurring from their present income, and that it would not be wise to forestall the income of the approaching session to provide for the deficit. He would suggest in future that they should either decrease the amount of their literature or increase the subscription, as there could be no doubt they were at present giving more for the money than they could afford to do. The Secretary replied that this question had been raised before, and that it had then been decided that to increase the subscription would place the Society out of the reach of the very class it was intended to encourage. As to the supposed deficit, though the assets could not well be placed on the credit side of the balance sheet, he had no doubt that when they came to be realized there would be a considerable balance in the Society's favour. The demand for the Society's publications had hitherto been very great, and even increased prices had been offered for numbers which were out of print. Last session such an emergency had been provided for by printing a larger supply, which had, of course, increased their expenses without in the mean time bringing in any return. The work of the Society had much extended, and it had been thought preferable to increase their sphere of usefulness rather than to lay up a reserve fund; though, looked upon even as a commercial speculation, he had every confidence in their ultimate success.

Mr. W. S. Franks, F.R.A.S., noticed with regret that Mr. Espin was obliged to remove from Liverpool, and would therefore be unable to undertake the duties of Special Observer. The fact, however, that he had purchased one of Calver's 17-inch reflectors looked hopeful, and he was sure Mr. Espin would continue to be one of their most valuable observers. (Applause.)

The Society then proceeded to the election of the President and Officers for the ensuing year, with the following result:—*President*: Isaac Roberts, F.R.A.S. *Vice-Presidents*: Revs. T. E. Espin and S. J. Perry, and Messrs. St. Quentin Gage and R. C. Johnson. *Special Observer*: W. St. Quentin Gage. *Treasurer*: G. C. Beecham. *Secretary*: J. W. Appleton. *Editor*: W. H. Davies. *Council*: Messrs. Baxendell (Sen.), Denning, W. H. Davies (Jun.), Elger, Franks, Gore, Knott, Newall, Sharp, and Thomas, Miss Brown, and Rev. J. H. Honeyburne.

Nice Observatory.

HAVING recently visited this Observatory, I trust a short account of it, with a description of the dome for the large equatoreal, now in course of construction, and which possesses some novel features, may prove interesting.

The Observatory and its extensive grounds occupy the summit of the hill known as Mont Gras, some 1200 feet above sea-level and distant from the Mediterranean about three or four miles.

On its southern slope is situated a set of buildings, consisting of dwelling-houses for the observers, library, calculating-rooms, &c., and higher up toward the summit are scattered the various buildings which contain the instruments. Amongst these may be specially noticed a fine equatoreal of about 15 inches aperture, with a very perfect driving-clock, the dome and walls wooden-lined throughout, and the shutters opening in sections; meridian and transit instruments of 6 and 3 inches aperture respectively, the roofs above which open right and left, running on transverse horizontal rails with a rain-tight central joint; also the usual instruments for noting force and direction of wind, &c.; all being of the highest class and in perfect order. A little lower and at some distance away is the building destined to receive the large equatoreal of 30 inches clear aperture, of very solid and massive masonry, and so far finished as to be ready to receive the dome foundations.

Owing to the large diameter of the dome, the outside diameter being 78·39 feet (six feet larger than the Pantheon at Paris), and its great weight, 96 tons, it became a very serious consideration whether the usual methods of support, namely wheels or balls, would be practically workable; and the question was submitted to the celebrated French engineer, M. Eiffel of Paris, who, after much thought, suggested that the dome should be made to float in a circular tank of water, and that running-wheels should be principally used as guides and take such a share of the superimposed weight only as might be necessary to secure perfect steadiness in rotation.

After much discussion the construction was placed entirely in his hands, and the work has been designed, carried out, and completed in his workshops, Paris.

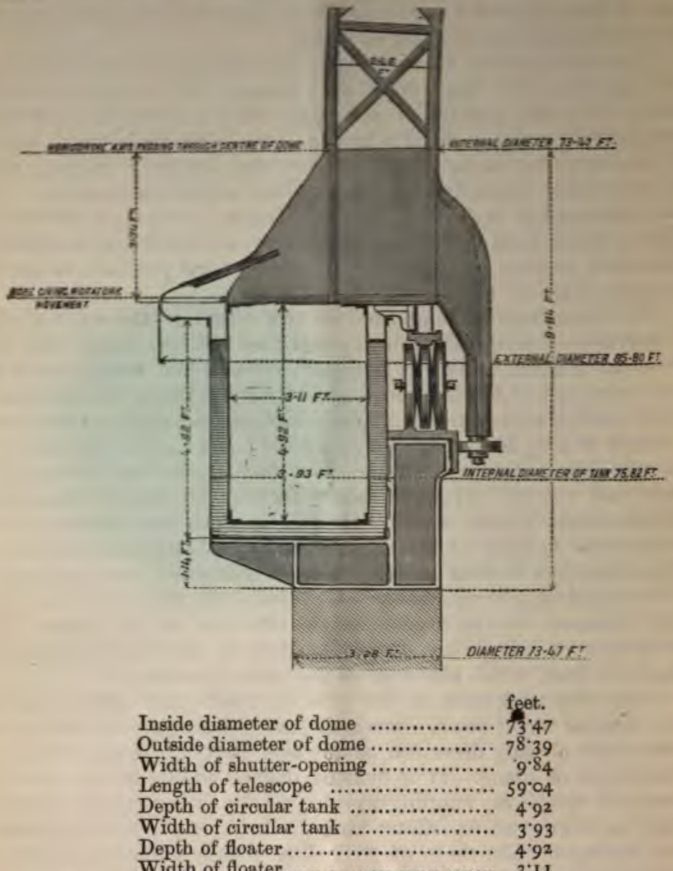
The writer was not only most courteously received and shown over the Observatory at Nice, but on calling upon M. Eiffel, an assistant was at once deputed to show the dome just then completed and to explain its construction and novelties.

It is made entirely of steel, so as to secure the greatest strength and lightness; the main framework consists of two semi-circular lattice ribs placed parallel to each other, about 10 feet apart, to which the shutter arrangement is attached; into these are butted a series of auxiliary ribs, 14 in number, which carry the outer sheeting, and this is further supported by intermediate angle-iron framing. To the lower end of each rib is secured a bracket carrying a horizontal wheel, which rolls against a circular cast-iron curb ring accurately machined, serving to retain the dome in central position so far as regards all lateral movement.

The dome itself is supported in two ways: on the walls of the Observatory a series of 36 heavy cast-iron saddles, shaped like the letter ┘ laid flat, is placed at equal intervals and firmly bolted to the stonework of the building; upon these saddles on the lower and outer portion a circular tank reposes, having a depth of 4·92 feet and a width of 4·05 feet. On the upper and inner part of the saddle is secured the circular cast-iron curb already spoken of, having two rails cast on its upper surface, carrying 36 sets of running-wheels, three wheels on a single axle in each set; those on the inside are flanged and serve to assist the horizontal wheels in preserving the central position; the outside ones have no flange. The central or intermediate wheels support an upper flat cast-iron rail, fastened to the underside of the ribs of the dome. All these sets of wheels are kept in position by internal and external bands of iron, which also receive the ends of axles.

From the underside of the ribs on their outer edge depends the circular inner tank, immersed nearly its entire depth in the outer tank already described, and which floats in it and supports the weight of dome. It is evident that by adding to the water in tank the weight on running-wheels can be either entirely or partially relieved, and by abstracting from it, any desired pressure can be placed upon these wheels; during high winds it is assumed it may be requisite to adopt this provision in order to secure increased stability. Again, should the tanks at any time need repair or require painting, the dome can revolve on its wheels alone. The force required to rotate the dome under normal conditions is extremely small; when once started a weight of 7 lbs. over a pulley will keep it moving.

The shutter is made in two leaves, and opens right and left with a rain-tight joint in centre; the clear aperture is about 10 feet, and each leaf runs on a rail attached to the top and bottom of the dome, and is made rigid by external lattice girders. In addition to this main shutter are supplementary internal ones, made of light material in sections of about 8 feet long, and pivoted on to each main rib, so that the space occupied by the main shutter can be closed down to the length of a single supplementary one or half of this.


HORIZONTAL AXIS PASSING THROUGH CENTRE OF DOME
INTERNAL DIAMETER 73·42 FT.
GIVING ROTATORY MOVEMENT
9·84 FT.
EXTERNAL DIAMETER 85·80 FT.
3·11 FT.
4·92 FT.
4·92 FT.
3·93 FT.
INTERNAL DIAMETER OF TANK 75·82 FT.
1·14 FT.
3·28 FT.
DIAMETER 73·47 FT.

	feet.
Inside diameter of dome	73·47
Outside diameter of dome	78·39
Width of shutter-opening	9·84
Length of telescope	59·04
Depth of circular tank	4·92
Width of circular tank	3·93
Depth of floater	4·92
Width of floater	3·11

Total weight of revolving part .	95 tons.
Total weight of fixed part	65 „
Total...	160 „

It may be interesting to note that it is intended to fill the circular tank with a solution of chloride of magnesium in water, having a density of 1·25, which will then resist a temperature of 40° Centigrade below zero. As a matter of fact, frost, except of the mildest form, is almost unknown in Nice; the very heavy density the solution materially diminishes the necessary cubic contents the circular tanks. Proper means are provided for giving access both these for painting and repair.

From an engineering point of view the mode of construction opted seems admirably suited to answer the purpose intended; t material will not last for ever, not even at Nice, and in spite of painting, oxidization will have its way sooner or later; but as is is in the future it is hardly worth while to meet difficulties fore they arise. We must all of us, however, not only admire the ought and skill which has been brought to bear upon this most teresting work, but still more the splendid and magnificent erality which has induced a private gentleman, M. Bischoffsheim, undertake, at his sole expense, the purchase of the property, the tablishment of an Observatory almost without a rival, its buildgs and instruments, and the permanent endowment of the same, ich it is his intention to make. J. L. Stothert.

udley, Bath.

Proper Motion of Lalande 16616.

(Communicated by Commodore G. E. Belknap, U.S.N., Superintendent Naval Observatory.)

E proper motion of this star has been deduced by comparing ervations of Lalande, Argelander, Robinson's places of 1000 rs observed at Armagh, and Washington Transit-circle observans in the years 1881 and 1882.

he Catalogue places are:—

Epoch.	α. h m s	δ. ° ′ ″
1800·0	8 17 16·11	+51 17 36·2
1842·0	8 20 22·27	+51 9 21·8
1870·0	8 22 25·58	+51 2 46·4
1881·0	8 23 14·38	+51 1 32·8
1882·0	8 23 18·76	+51 1 21·35

hese observations, reduced by precession alone to 1882·0, e:—

Date of observation.	Epoch.	α. h m s	δ. ° ′ ″
1800·075	1882·0	8 23 19·44	+51 1 51·14
1842·465	„	19·12	36·37
1870·426	„	18·56	25·88
1881·216	„	18·79	21·06
1882·168	„	18·76	21·35
an 1855·270		8 23 18·954	+51 1 31·16

Giving these observations equal weight, forming equations of

[illegible]

$$[illegible]$$

whence

$$[illegible]$$

[illegible]

[illegible]

Annual motion in R.A. = [illegible]
Annual motion in Dec. = [illegible]

Edgar Frisby,
Prof. Math. U.S.N.

[illegible]

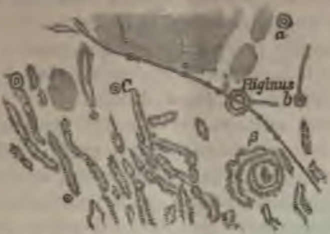
Hyginus N.

A short time since, I had given to me by a friend a small engrave map of the Moon, 7 inches in diameter. It evidently formed par of some elementary book on astronomy, but was loose when he ha it. The points of the compass are in English—"North," &c. Th inscriptions are "J. A. Barra. dir. L. Giguet del. et sc." It wa evidently of French origin, and the orthography, where not Latin is French. In the neighbourhood of "Huygens" (*sic*) I foun some suspicious-looking markings, which induced me to prosecut further inquiries. On showing the map to Mr. Wesley at the Royal Astronomical Society's rooms, he identified it as a reproduction of one in Arago's 'Astronomie Populaire,' in which it is referre to as follows (Arago, Astron. Pop. vol. iii. p. 444):—

"Enfin, l'astronomie est en possession d'une carte lithographiée de 95 centimètres de diamètre, fruit des études persévérantes de MM. Mædler et Beer. J'ai chargé M. Barral de surveiller, pour cet ouvrage, l'exécution d'une réduction de cette carte (fig. 296, pp. 448 et 449). Elle est dessinée renversée, telle qu'on voit la Lune dans les lunettes astronomiques."

We then referred to Beer and Mädler's original large map of 1834 in the Society's Library (the same that Mr. Gill used at the

Cape), and there we found, in the proper place of Hyginus N., an oval marking, which fairly corresponds to the object as indistinctly seen or badly illuminated. A comparison of the region as shown in the map of it, with that figured in the 'Observatory,' vol. v. plate ii., shows a good accord in the position of most of the features.



I enclose a tracing from the map kindly made for me by Mr. Wesley, as perfect as he could get it from an old map varnished.

Mr. Wesley informs me there have been considerable alterations in the region in the later editions, but that the special marking remains unaltered. He describes it as little more than a slight outline marking covered with dots like the rest of the ground, and which dots he has left out for clearness. He adds that on looking at it in the later editions of the map (which are not varnished) it seems to be formed of stronger dots than the general surface, and "looks intended for a slight hill." Now we know that this appearance of a hill is presented by Klein's object under certain conditions of illumination; and although the oval mark in Beer and Mädler's map only roughly corresponds with our modern drawings, it does seem to me its position and general appearance are near enough to raise a serious question as to the big N (Nova?) applied to Dr. Klein's object.

I am not sufficiently acquainted with the literature of the subject to know whether this point has been raised or discussed before. If not, it may possibly open a new interest to a matter of which we have heard little lately.

J. Rand Capron.

Guildown Observatory,
1885, July 18.

Schellen's 'Spectrum Analysis' *.

For the last thirteen years the first English translation of Dr. Schellen's work has enjoyed a most widespread and well-deserved popularity as a standard textbook of spectroscopy. But though the work was most complete as a record of the state of spectrum analysis in 1872, the date of its publication, yet so much has been achieved since which needed to be recorded, that some years ago a further edition had become obviously necessary. Dr. Schellen therefore resolved upon a thorough reconstruction of the book, and was engaged upon it when he was attacked by the illness which unhappily proved eventually fatal. The task he had undertaken was not, however, left unfulfilled, but was carried to completion by his friend Dr. H. J. Klein.

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* 'Spectrum Analysis in its Application to Terrestrial Substances, and the Physical Constitution of the Heavenly Bodies.' Familiarly explained by the late Dr. H. Schellen. Translated from the Third Enlarged and Revised German Edition by Jane and Caroline Lassell. Edited, with Notes, by Captain W. de W. Abney, R.E., F.R.S. With numerous Woodcuts, Coloured Plates, and Angström's and Cornu's Maps. Second Edition. London: Longmans, Green, & Co. 1885. Pp. 626.

This third German edition (for the first English edition was the translation of the second German) appeared in 1883. The Misses Lassell, who had translated the earlier one, and to whom English readers owe no slight and meagre gratitude for bringing so valuable a work within their reach, readily undertook the translation of this edition also, while Capt. Abney undertook the duties and responsibilities of Editor, and revised, and to a certain extent remodelled, the whole. It will be seen, therefore, that the present volume is a new edition in a higher sense than is sometimes meant by the term, and calls for a much more detailed criticism than a mere corrected reprint would do.

There are two points of view from which the present work might be regarded. It may be compared with the earlier edition, and the question asked if it as fully and adequately represents the present state of spectroscopy as that did spectroscopy in 1872. We fear in this case the comparison would be somewhat to the detriment of the later work. Thus the chapter on the absorption of light by liquids seems decidedly incomplete; no reference, for instance, is made to so interesting a matter as the relations of the absorption-bands of the various salts of didymium and cobalt, and the shifting of the bands of chlorophyll is dismissed in a three-line note. In solar work the information supplied as to Lockyer's recent investigations is summed up in the statement that the latter has communicated "several important papers" to various societies. The record of the results of observations of total solar eclipses is decidedly meagre; Liveing and Dewar's laboratory researches are scarcely even referred to, and Ciamician's theory as to the spectrum of cyanogen is unnoticed. No omissions of equal importance could at the time of publication be charged on the earlier work.

But, on the other hand, it is but fair to admit that the difficulty of producing so complete a work as was the first English Schellen has immeasurably increased; not only does the science cover a much more extensive field, but its condition is not so convenient for reporting progress upon as it was in 1872. In the first ten or twelve years after Kirchhoff's celebrated observation of the reversal of the sodium lines a great number of branches of spectroscopic research were opened out, and followed up so eagerly as to be soon almost exhausted—on every hand something like a definite tangible result had been arrived at. But at the present day it is on many points by no means so easy to sum up the results of the investigations still in progress; and it is probably the difficulty of treating questions which are still matters of controversy which has led to the omission of any full notice of the above-mentioned researches. It will therefore be fairer to judge the book by estimating its original value and the value of that which has since been added to it, and, so doing, there seems ground for little but the most cordial praise. The first edition admittedly gave a most complete and accurate representation of the then state of the science; we find it now much expanded, carefully revised, and to a very considerable extent brought up to date. Great praise is due to the Editor for the judicious way in which he has secured space for many valuable additions by remodelling the earlier part of the work. The portions left out in the two first parts were none of them essential, and the alteration of the order of some of the sections is a decided improvement. Either the Translators or the Editor, or both, must be allowed great credit, too, for the compression of not a few long German paragraphs within the limits of a short English sentence. The saving of space, if clearness be not sacrificed, and that condition seems to have always been fulfilled, is a gain in every way.

Going through the work in detail, and comparing it with the first English edition, we find that the First Part, "On the Artificial Sources of High Degrees of Light and Heat," has been slightly condensed, to the decided gain of the book. The Second Part, "Spectrum Analysis in its Application to Terrestrial Substances," has been very greatly expanded, the theory of refraction, dispersion, and diffraction being treated much more fully than in the first edition; much additional information is given as to the different forms of spectroscope, and especially as to grating spectroscopes. A good account of Prof. Rowland's concave gratings is added by the Editor. The spectroscope of Thollon is, however, passed over, the Christie half-prism spectroscope only mentioned, and Fievez's combination of it with a grating not referred to. The accounts of Langley's bolometer work, of Abney's photographs of the infra-red spectrum—the Editor has added many valuable pages in this part of the volume—of the work done by photography in the mapping and comparison of spectra, of recent researches on the ultra-violet and fluorescence spectra, and in spectrum photometry, are all new matter, and fully maintain the reputation of the book.

In the Third Part, "Spectrum Analysis in its Application to the Sun," the discussion started by the late Dr. Henry Draper, with regard to bright lines in the solar spectrum due to oxygen, is fully and fairly reviewed. "Rainband" spectroscopy is the next new subject, and is clearly dealt with. The study of the Sun's surface comes next; and several recent observations are given at length, though, as stated above, we miss any satisfactory reference to Lockyer's work, despite its importance from more than one point of view. In the following sections the Editor has made several important alterations: the descriptions of various photographic apparatus are much curtailed, but a very good description of the Dallmeyer photoheliograph is substituted. The account of total solar eclipses has been sadly curtailed, and affords the only instance in the body of the work in which we think the editorial pruning-knife has been too freely used. The results of the observations of the eclipses of 1882 and 1883 are, however, summarized, but the diagram, fig. 175, illustrating these is incomplete and insufficiently explained. Capt. Abney likewise gives a short but clear description of Huggins' method of photographing the corona without an eclipse. A curious expression occurs in the last paragraph of this section, when the supposition that the corona is partly meteoric in character is called a "bold conclusion" of Schuster's. The theory had been current among astronomers long before Schuster's paper, the very merit of which lay in the fact that it brought solid support to a theory which had been formerly accepted without perhaps sufficient reason. The last two sections of the Third Part are new, and deal with the determination, by means of the spectroscope, of the speed of the Sun's axial rotation, and of the absorptive action of the solar atmosphere upon rays of various refrangibility.

The Fourth Part contains some valuable additional matter on stellar spectroscopes, amongst which Vogel's modification of Zollner's eyepiece-spectroscope deserves especial notice. Additional details are also given of the spectra of the planets. The positions of only three bands in the spectrum of Neptune are given. Maunder has succeeded in measuring the positions of six, which probably correspond to six of the darkest bands in the spectrum of Uranus. It might be mentioned here that the discovery of the broad lines near *b* in the spectra of sun-spots, p. 309, the observations of Comet Wells recorded on pp. 516, 517, and 518, and the observations of the proper motions of stars made at Greenwich, should have been credited to this observer.

In the Fifth Part, which deals with the spectra of the Fixed Stars, the principal additions are:—Vogel's classification of star-spectra (where Vogel has been followed in denying the hydrogen lines to β, γ, δ, and ϵ Orionis; but this is certainly a mistake); Huggins' photographs (an important section); observations of variable stars, of the wonderful star Nova Cygni 1876, and of the results obtained at Greenwich as to the motions of stars in the line of sight. In the remaining parts, observations of Comet Wells—the great comet of 1882 is not mentioned—Huggins' photographs of cometary spectra and recent observations of the spectrum of the aurora are the most noticeable of the additions. The determinations of the positions of the bands in cometary spectra here given are not the best available; comets *b* and *c* 1881, which are not mentioned, having supplied the most accordant values we have.

The Appendices are very valuable. The first five are given in the body of the third German edition, and likewise the last. The seventh, eighth, and ninth contain complete tables of wave-lengths, for the dark lines in the solar spectrum, for the metals, and for the non-metallic elements. In the seventh appendix, where Angström's determinations of the wave-lengths of the lines in the solar spectrum are given, the actual measures have been wisely omitted and only the results retained. It should be noted that the values for the wave-lengths of the principal Fraunhofer lines in this table do not quite agree with Ångström's *definitive* values, some small corrections not having been applied. Amongst the plates are a beautiful copy of Ångström's Normal Spectrum, Cornu's map of the ultra-violet, and Huggins' photographs of stellar spectra. We miss Kirchhoff's map, which was given in the former edition. It was the more necessary that it should have been given, as when Kirchhoff's numbers are mentioned the equivalent wave-length is not stated; and as there is no map given, the reader is quite unable to form any idea as to the real place of the lines. Appendix B of the first edition might well have been retained, whilst it must be matter of great regret that the Editor has omitted the bibliography of the 1872 edition instead of trying to bring it up to date. A catalogue of the brighter stars, with the types to which they belong, would have considerably increased the usefulness of the book, and would have occupied but little more than a page of matter.

The short editorial footnotes scattered through the book are often disappointing, some are unnecessary, others would better have been omitted. This is the more remarkable since the general editorial work is well executed, and the paragraphs introduced into the text are clear, straightforward, and concise. The Translators have carried out their portion of the work not less ably than in 1872; the translation is free, yet sufficiently faithful, and the English is easy and with scarcely a trace of German style or idiom. So far as a first rough examination has gone, there would appear to be very few mistakes. Two may be mentioned. On p. 445, line 21, "the lines of hydrogen and the line D_3 are distinctly visible," should have been "the lines of hydrogen appear *bright*." The very peculiarity of the class of stars in question is otherwise missed. On the following page, line 2, there is an error of precisely the opposite character; speaking of spectra like that of Capella, it is said, "the metallic lines are very numerous and are of sufficient brilliancy," "*Intensität*" being rendered "brilliancy" instead of "intensity;" the lines in question are dark absorption lines.

The book, though containing so much more matter than the 1872 edition, contains no more pages. This result has been obtained by using a type not quite so large as before, though still sufficiently bold and distinct. The plates have been very carefully executed; and the paper, printing, and woodcuts are such as to render the appearance of the book all that can be desired.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Jupiter and Large versus *Small Telescopes.*

Gentlemen,—

It is difficult to account for the discrepancies in the observations by different observers. And when it is seen that these discrepancies relate to markings of prominent character, and that the results are so variously interpreted, we cannot wonder that we are still far from having acquired anything like a comprehensive or unanimous knowledge of Jovian phenomena. The contradictions alluded to are to be regretted, though it is hard to see how they can be eliminated in face of the widely diverse conditions controlling individual observers, and the ideas and prejudices which more or less influence our judgment.

Here is Dr. E. J. Spitta saying, in reference to the equatoreal white spots, "I cannot help thinking they have a proper motion of their own—an old idea, I allow, but still one which even yet has not been definitely proved, I think" ('Observatory,' August, p. 274). After several thousands of observations by Barnard, Corder, Hough, Kortazzi, Schmidt, Terby, Williams, myself, and many others during the last few years, all consistently and absolutely demonstrating the fact that these white spots have a proper motion of slightly more than 8° per day (=about 6000 miles) relatively to the red spot, one would have thought that Dr. Spitta, coming so recently to the study of Jupiter, would have honoured us with a little more confidence. If he will but look again for these white spots early next opposition, he will unmistakably see them threading rapidly along the equatoreal border of the great southern dark belt, and completely passing the red spot in little more than four days. These phenomena are so very palpable that a child may witness them without much effort.

The junction of the faint dusky narrow belt S. of the red spot with the southern border of the spot appears to have eluded Prof. Pritchett both in 1884 Jan.–Feb., and 1885 May. I have quoted the conclusive evidence of five perfectly independent observers ('Observatory,' March 1885, pp. 77, 78) as to the coalition of the belt and spot in 1884, and have no desire to similarly corroborate the fact as observed in 1885. It will suffice if I reiterate that on May 9 last a faint narrow belt lying to the S. *p.* side of the red spot was bent abruptly down to that object and actually involved with the dark material of the spot. The connection was not seen as a diffused mass of shading, but as a narrow and very definite vein of dark matter crossing the white zone outlying the spot. On that night definition was very sharp and the details were superbly brought out by my newly silvered mirror, powers 252 and 475. I endeavoured to make a sketch and fill in some of the mass of detail diffused in wonderful variety over the surface, but rotation so quickly displaced the aspect of the disk that I found it impossible to carry out my intention. The bright space contiguous to the N. *f.* boundary of the spot which Prof. Pritchett calls a "fine white border" is always plain in my telescope, and the spot never appears, except under very bad definition, as nearly touching the belt in this direction. The intervening space is sufficiently wide and brilliant to be always obvious. Such other details as are mentioned by Prof. Pritchett as visible in his 12½-in. O. G. are readily seen here, and some additional ones, including the bright longitudinal division in the great S. belt, which, curiously enough, is not represented at all in his sketch. The latter also omits the very dark *f.* end of the spot (though observed by C. W. P., Jun.) which has been visible for some months in any small telescope.

While your correspondent agrees with me as to rapid changes on Jupiter, I must entirely dissent from his views that the red spot had a sudden formation and at first a large proper motion*. Unquestionably the spot may have become wonderfully intensified during the first half of 1878. As to proper motion, does Prof. Pritchett allude to that he describes in the 'Observatory' for Jan. 1879, p. 308? when he says "The most remarkable phase of all was the rapid *proper motion* of the 'elliptical cloud' [*i. e.* the red spot]. I watched it for nearly one hour, and in that time its front moved on the disk of the planet about one fourth of the planet's diameter. It passed by the most easterly of the spots, and its front had reached to the most westerly spot and was passing it at $12^h\ 5^m$ Gl. M.T." In this case the alleged proper motion was simply the effect of rotation! and as to the red spot catching up and passing the white spots near the equator, this is a phenomenon perfectly inexplicable and one unique of its kind. The mass of evidence accumulated in recent years proves that it is the white spots which overtake and pass the red in the direction of rotation. That a displacement could occur of the startling character described, and under the very eyes of the observer, is open to the gravest doubt, as it is opposed to all the knowledge gained of Jovian phenomena in modern times. As to whether the red spot, during the few months immediately following July 1878, exhibited a see-saw motion, causing it to oscillate between wide extremes of longitude, or whether the spot rapidly varied in its visible aspect, is also very doubtful, though it must be confessed that the observations of Dennett, Niesten, Pritchett, and Trouvelot are as discordant as they well can be. On July 9, 1878, Prof. Pritchett estimated the longitude as 247°, while Dennett on July 27 found it 204°. On Aug. 6, 1878, Niesten gave it as 194°, and two nights later found it 238°! On Aug. 21

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* Relatively to the true motion of the material sphere of Jupiter (the precise rate of which we are ignorant) the red spot probably has a slight proper motion, though none of the marvellous changes shown in the velocity of the equatoreal white spots. Such variations as apply to the red spot are spread over comparatively long intervals and appear to be far more gradual in their operation. All the more reliable observations prove this.

he fixed it at 185°, on Sept. 2 at 243°, and on Sept. 9 again as 185°! One is tempted to suggest there were two spots. Sometimes the object eluded observation when it should have been central; on other dates it was conspicuous close to the limb. All this is very perplexing and can hardly be explained by large errors of observation. It is true, however, that the positions of the spot were *roughly estimated from sketches,* which are rarely accurate in fixing places, and that on occasions of bad seeing the spot may either have eluded notice altogether or its place have been very inexactly assigned. In presence of the doubts affecting the initial observations we are scarcely warranted in assuming this enormous feature to have shown the remarkable irregularities of motion ascribed to it. It is almost universally admitted that the spot is an old feature affected by singular modifications of appearance which now and then throw it into prominent notice. But there has never been sufficient proof of a rapid oscillation in its velocity. The early observations in the summer of 1878 may be difficult to reconcile unless by "dodgy" behaviour on the part of this well-known object; but we are hardly justified in adopting such an extraordinary view, especially if we regard atmospheric conditions as affecting our judgment to the extent implied in your correspondent's opening paragraph.

I entirely agree with Prof. Hough in the views he now propounds ('Observatory,' Aug. 1885, p. 276) respecting the changes on Jupiter. The statements occasionally made as to new belts appearing in a few hours are simply the effect of the swift rotatory movement of the planet. Prof. Hough allows that "minor changes occur from day to day," while certain features, such as the red spot, are sustained for some years with little apparent alteration of aspect. There will be few to dissent from this opinion. Objections have been previously raised to Prof. Hough's opposition to the idea of sudden changes from his remarks in the annual reports of the Dearborn Observatory, in which a general statement was made that "the changes taking place are slow and gradual;" but his views, as now more definitely expressed, are quite in accord with those of others who have recognized, over and over again, the great unstability of certain lineaments on the planet's surface.

Prof. Hough, commenting upon the blending of the spot and belt in Jan.–Feb. 1884, says:—"I have no doubt that with a small telescope I should have seen just what others saw, as the spot was apparently against a faint belt following, but at no time was it 'merged into the belt.'" But Prof. Hough quite overlooks that his argument is vitiated by the fact that Mr. Green used a reflector of 18-in. aperture, and that his observations (with a telescope of nearly the same capacity as the Dearborn reflector) are the most conclusive of all relatively to the disputed junction of the spot and belt. This junction existed irrespective of the size or character of telescope or of atmospheric interference, just as it existed (with some little modification of detail) when I saw it under splendid definition in May of the present year *. To attribute the connection of these markings as an apparent effect due to inadequate aperture, is therefore utterly futile when confronted with Mr. Green's results. Moreover, has it not occurred to Prof. Hough that the small instruments to which he adverts ought to have shown the division with Jupiter at opposition as they exhibited it plainly enough with the planet near quadrature and far less favourably placed? The argument which he also applies in the sentence "but the fact that subsequently the spot was seen by everybody separate and distinct, leads us to infer that it did not coalesce at any time," is equally devoid of weight. Indeed it is at once apparent that the point alluded to by Prof. Hough furnishes very strong evidence the other way. With Jupiter at maximum diameter and other favouring circumstances, the highest magnifiers failed to separate the spot and belt, yet, later on, this was accomplished readily enough. To this but one logical explanation can be found, viz. in the actual separation of the two features.

In this case the reflectors seem to have possessed an advantage over refractors. The suggestion is affirmed by the fact that in Sept. 1883 Mr. Williams could distinguish the outline of the spot well with a 5¼-in. Calver, whereas at Palermo with the 10-in. refractor the place of the spot appeared "very white"! The question involved is an important one, and the ensuing opposition will possibly enable us to solve the difficulty. The singular appearances now in dispute will probably be reproduced; and if observers generally will critically scrutinize the southern boundaries of the spot during the coming winter, the junction of the narrow belt on the S. side may be again observed. I have referred at length to this matter because it is not only most essential such contradictions should be cleared up, but the fact of the spot being joined to a branch of the dark belt nearer the pole, struck me as very remarkable when I first observed it, seeing that the narrow belt on its equatoreal side has specially avoided the spot and actually runs into the great S. equatoreal belt in order to escape the coalition. In explanation of the latter peculiarity Prof. Hough has ingeniously suggested (Report of Dearborn Observatory, 1883, p. 10) that "the non-blending of the two objects would seem to indicate that they are composed of matter having repellent properties, similar to two clouds charged with the same kind of electricity."

The dark reddish spots, referred to by Mr. Green as situated in the same latitude as the old red spot, have been very generally observed during the past opposition. They are elongated in the direction of the belts, and the principal spot, which follows the

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* It was also strongly suspected by Mr. Barnard on April 25, 1885. He says:—"There was a mass of darkish shading *s.p.*, possibly in contact with the south edge of the spot" ('Observatory,' June, p. 211). I have invariably observed the connection of the spot and belt to the greatest advantage when the seeing was very good and the minor details visible with unusual effect. This is quite opposed to the theory that it is a false appearance.

great red spot $1^h\ 48^m$ and maintains precisely the same rotation period, has been visible since the morning of Sept. 27, 1883, when it was seen by Mr. Franks at Leicester with a 11¼-inch reflector. He described it as "a faint elliptical spot devoid of colour or strength of outline," and gives a sketch in the 'English Mechanic' for Oct. 12, 1883, p. 126, in which the spot is figured, not only under the same form and size, but in the same latitude and longitude as at present. The only dissimilarity is in the increased depth of tone and more prominent aspect of the marking seen during the recent opposition. There can, however, be little doubt of their absolute identity, or that these minor red spots are of considerable permanency, so that we may confidently predict their reappearance, during the ensuing autumn, when the planet again becomes observable.

Mr. Barnard's interesting reference to the spots on Jupiter ('Observatory,' June, 1885, pp. 210–11) has led me to associate his transits with those of similar markings observed here. The white equatoreal spot he noted central on April 20 at $10^h\ 1^m\cdot5$, and on April 25 at $7^h\ 56^m$ (Nashville M.T.), was the same object which has been so persistently visible during the last six years, and which he himself has often observed on previous occasions. The "sharp indentation" he describes in the "outer edge of the N. equatoreal belt" on April 20 and the "large, luminous, ill-defined spot" seen in the same position on April 25 were identical with each other and with a feature which has been followed here since January 27 last. I append the observed times of transit, including Mr. Barnard's two estimates, reduced to G.M.T.

Large white spot indenting N. edge of great N. belt.

		h	m
Jan.	27....	11	1
Mar.	4....	10	21
	7....	7	54
	11....	11	4
	12....	6	57
	14....	8	29
	28....	10	3
April	2....	9	8
	4....	10	45
	12....	7	19
April	18....	12	20
	19....	8	7
	20....	13	49·7
	23....	11	23
	25....	12	55·2
May	1....	8	0
June	1....	8	33
	3....	10	18
	13....	8	19
	20....	9	11

Rotation period $9^h\ 55^m\ 31^s\cdot5$, or slightly less than the red spot. There were several other large white spots in the same latitude. Mr. Barnard obtained a pair of observations of another of these on April 22 and 24.

With regard to the general question as to the comparative merits of large and small telescopes, it appears that Professors Young and Hough and Mr. Green agree in asserting that with a large instrument they can see everything visible in a smaller one and see it better. This is important, as proving there is some practical advantage, slight though we may fairly consider it, in favour of large aperture. But they do not tell us they can see anything in the big instruments which is invisible in the smaller, though I suppose that is left to be inferred? The question is one which, I apprehend, will be best settled by comparisons, side by side, with telescopes of different apertures. Large instruments have so frequently proved disappointing on planetary markings, while, on the other hand, small ones have often surprised us by their effective grasp of detail, that the assumed superiority of the former may well be questioned. In any case the advantage claimed for the costly, giant telescopes of the present day, if really admissible, must be so very trifling that we are justified in doubting its significance, especially on bright planets, which do not essentially require excessive command of light. With respect to other classes of objects, such as very faint telescopic comets or nebulæ and minute stars and satellites, the necessity for great penetrating power is not disputed. The efficacy of small instruments comes in where it is desirable to have that critical sharpness of the image resulting from a suitable blending of aperture and power with atmospheric conditions.

Yours faithfully,
W. F. DENNING.

Bristol, 1885, Aug. 3.

Kirch—an Astronomical Family.

GENTLEMEN,—

Perhaps a few more particulars respecting this scientific family than space allowed in my last may be acceptable to some of your readers.

Gottfried Kirch was born at Guben, in the district then known as Lower Lusatia, on the 18th of December, 1639. His father, a tailor, was obliged, in consequence of the troubles arising out of the thirty years' war, to take refuge with his family in Poland. Young Kirch, however, who early showed a taste for mathematics and astronomy, found means to complete his studies at Jena, and was afterwards recommended to Hevelius, whom he assisted for some time at Danzig. Whilst residing at Coburg in 1680, he was the first to discover, on Nov. 14, the celebrated comet of that year. It was not until 1700 that he took up his abode in Berlin; and on the establishment of the old Royal Observatory (which formed part of the buildings of the Royal Academy of Sciences) in that city, by the first King of Prussia, in 1705, he became the first Director, and died there on the 25th of July, 1710. He married his second wife, Maria Margaretha (who was born in 1670, and the daughter of a clergyman named Winckelmann), in 1692; this lady was of the greatest assistance to him in his work, both the calculations and observations; she wrote, moreover, several astronomical papers herself, and discovered a comet (the second of that year) in April 1702. They had a large family, of whom two, a son and a daughter, shared the scientific tastes of their parents. The former, Christfried Kirch, who was born at Guben on the 24th of December, 1694, became Director of the Berlin Observatory in 1716, and remained there until 1740, when he died on the 9th of March; as mentioned in my previous letter, it was he who discovered the comet of 1718. His sister, Christina Kirch, assisted him both in his calculations and observations, as their mother had done their father; she was about two years his junior, and lived to a great age, dying at Berlin on the 6th of May, 1782.

Yours faithfully,
W. T. Lynn.

Blackheath, 1885, Aug. 18.

Biela's Comet.

Gentlemen,—

In the 'Observatory,' No. 100, page 263, I have expressed myself so badly on the subject of the possible place of Biela's Comet in 1872, that I must beg of you to allow me to state that I meant to say to Mr. Denning (the letter was never intended for publication) that the perturbations of the Comet had not been carried forward to 1872 and that there was a possibility of the retardation in question being accounted for, not as having taken place in the six preceding years, but during the three revolutions. Any one desirous of knowing exactly how the case of Biela stands, should read the communication of Mr. Hind to the 'Monthly Notices,' 1873, March.

Although the coincidences of periodic time and line of nodes are most curious, I should not like to say that there is any great probability of Denning's Comet being identical with Biela's. I only admit the possibility.

G. L. Tupman.

Hillfoot Observatory, Harrow.

Close Planetary Conjunctions.

Gentlemen,—

The closest planetary conjunction of this year is described in the following formal manner in the 'Nautical Almanack' under July 17:—2^h ☿ ☌ ♀ ☿ 0 11 S. The objects were readily visible in the same field of view of a 3¼-inch this afternoon, but being separated by a third of the diameter of the Moon did not afford a very striking conjunction. Mercury and Venus seem to have furnished the only instance observed of one planet occulting another, viz. in the year 1737. A close appulse probably occurred shortly before the invention of the telescope. In 'Scientiæ Eclipsium a Melchiore a Briga' (pars iv.), p. 147, we have the following:—"Mercurium à Venere occultatum an. 1599 d. 8 Junii ex suâ observatione putavit Michael Mœstlinus, et consentit Keplerus in opticis." The author goes on to refer to the case of 1737, in words that show some observation of the planetary occultation was obtained at Paris as well as at London:—"Idem spectaculum renovatum est an. 1737 die 28 Maii, quando Parisiis D. J. Cassinus vidit Mercurium tangere limbum occidentalem Veneris, a quâ incipiebat obtegi h. 9 30′ 3″ p. mer. deinde vapores observationem prosequi vetuerunt." The appulse or occultation of interest above all others would manifestly be one of Saturn by Jupiter: no such has occurred in the annals of telescopic observation; and the only close one I find previous to the telescope is mentioned in the same work (a Melchiore à Briga) p. 177:—"An. 1563 die 24 Augusti hor. 14.30′ Aurange, Junctinus vidit Saturnum australiorem in gr. 28 Cancri quasi cooperiri à Jove, ut ipse narrat in Præfatione ad suas tabulas."

Faithfully yours,
S. J. Johnson.

Melplash Vicarage,
Bridport, 1885, July 17.

Stellar Photometry.

Gentlemen,—

In the July number of the 'Observatory' you give a report of a somewhat animated discussion, by the members of the R.A.S., on a communication of mine to the Society, on certain points relating to Stellar Photometry. That paper was not intended by me for an extempore discussion, because I was aware that it referred to topics some of which were not obvious, and others required a deliberate consideration; otherwise it would have been my duty to have been present at the reading of the paper. The Society, however, is greatly indebted to the Astronomer Royal for the admirable explanations which he gave of the several difficulties as they were successively propounded by some of those who took part in the discussion. So far my absence was really a gain to the listeners; and beyond that his valuable remarks are now accessible to the readers of the 'Observatory.' Nevertheless, as there are a few points which still require explanation, I shall request a little space whereby I may hope to remove any erroneous impressions which may still possess the minds of astronomers interested in the subject. I need hardly say that it will be desirable for the reader to have at hand the last numbers both of the 'Monthly Notices' and of the 'Observatory.'

Mr. Knobel and some others with him, owing, I fear, to some inadequate explanations of mine, both personal and written, have been, it seems, led to the impression that by means of a certain screw attached to the photometer, the observer by a rapid and possibly habitual motion of the screw, might continuously move the wedge up to the same point which he had persuaded himself was the point of extinction of the star. I cannot help thinking the suggestion is at least as ingenious on the part of the objector, as it would be on the part of the observer to succeed in such an act of unconscious legerdemain. But as he would virtually change his instrument by altering the aperture of his telescope, it would not lie within the range of probability that he could make his two sets of independent measures accord in the way they ought and actually do accord. Be this as it may, there is a still more cogent reason for dismissing all doubts as to the mysterious action of this screw, for Mr. Plummer very rarely if ever uses the screw, and no such is attached to the photometer used by Mr. Jenkins. Intention and attention, combined with sufficient experience in the use of this extremely simple instrument, are the only secrets in the production of the accordant results.

I regret that the evidence for the point of extinction of Polaris being unaffected by moonlight was received with some incredulity. But the evidence for the fact is overwhelming, as demonstrated in the course of many thousand extinctions of the star in every phase of the Moon. Moreover, the cause of it seems obvious, and it was clearly explained by the Astronomer Royal. To Mr. Howlett I am obliged for the question as to the quite different effect which would be produced by bright sunlight; his question has led to some experiments of a quite successful character in relation to the whole subject, and these I may probably communicate to the Society at a future period. It is in results of this sort that the utility of the evening discussions may be traced.

General Tennant appears to have been under the impression that occasionally a whole night's measures have been suppressed on account of discordances, and that unintentionally I have led him to that misapprehension. I regret it if I have so misled him, but the reply is sufficiently simple. In vol. xlvii. of the Society's Memoirs I have given the results of some twelve thousand extinctions. I have in connection therewith suppressed not a single measure, all are equally utilized; but I believe I should have acted more wisely if I had suppressed several measures taken in unfavourable weather, and in the revision of the work now in the course of printing I shall certainly suppress some, though never without notification.

As to whether or not I have made an improper or injudicious selection from the measures in the Harvard Photometry, I thought and still believe that I gave the entire table of the Harvard measures of all the brighter stars down to Polaris, taken from page 406 of the Harvard volume. My object was to inquire, from the records published in that remarkable and valuable volume itself, whether three determinations of a star's relative magnitude were necessarily sufficient to secure accuracy. The facts of the case lie before the impartial judgment of the reader.

I was also sorry to read the opinion expressed by one of our eminent Fellows to the effect that he did not believe in either the meridian or the wedge photometer. This also was my own mental attitude some four years ago, but long subsequent experience with the latter has entirely changed my opinion. The double image or polarizing photometer originally devised by Zöllner and modified in the Meridian Photometer by Prof. Pickering is a very effective instrument of extreme beauty. The Wedge Photometer as I have modified and applied it is of extreme simplicity. The probable error of the observations made with the Zöllner photometer is reported by Dr. J. Wilsing of Potsdam to be much greater (0·053 mag. as against 0·035) * than that deduced from the Wedge obser-

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* See 'Astronomische Nachrichten,' vol. 109, p. 50.

vations, and this is confirmatory of my own experience. Be that as it may, each is trustworthy and valuable in its own line of proper application. But, after all, it is the eye which is the actual photometer; the optical instrument effects little more than to facilitate the means whereby the eye or the brain may record its judgment. Many experiments lead to the conclusion that this judgment is very limited in extent, when applied to light which in its character is either feeble or intense. Photography, no doubt, presents the promise of future usefulness, but I fear it also will be attended with similar limitations.

As applied to the stars, the experience of four years leads to the conclusion that either form of photometer (*i. e.* of the Wedge or any form of the Zöllner) properly applied on any single fine night may be reasonably expected to furnish the apparent relative magnitude of a star with an error not appreciably exceeding the tenth of a magnitude.

University Observatory, Oxford,
1885, July 17.

Your obedient Servant,
C. PRITCHARD.

NOTES.

LANGLEY'S RESEARCHES ON SOLAR HEAT.—The results of the work upon which Prof. Langley has been engaged for the last several years have recently been published in a complete form as Volume xv. of the Professional Papers of the United States Signal Service. No work more thoroughly aggressive, more calculated to further the progress of science, and to render it secure, has been undertaken during late years than that which Prof. Langley has carried out chiefly by means of his ingenious and useful instrument, the Bolometer. We have had occasion to refer to Prof. Langley's work on former occasions, as in our number for last February, and in the report of the meeting of the Royal Astronomical Society held on April 10 we gave Prof. Langley's own account of his researches. It will therefore only be necessary for us to give a brief outline of the present volume. The first chapter contains the preliminary observations made at Alleghany, from which Prof. Langley deduced the value of the Solar Constant as 2·84 calories, a much larger quantity than that generally accepted hitherto. These observations also showed him that, contrary to the generally received opinion, the greatest absorption occurred in the visible portion of the spectrum, particularly in the green and blue, and that it diminished as the extreme infra-red was approached, and convinced him that the labour and expense involved in repeating the observations at the base and at the summit of a lofty mountain would be well repaid by the gain in our knowledge. The generosity of a citizen of Pittsburgh supplied the means. In July 1881, therefore, Prof. Langley started for Mount Whitney, a lofty eminence in the Sierra Nevada of Southern California, over 14,000 feet in height, which seemed to combine all the needed requirements. The second chapter gives an interesting account of the journey to Mount Whitney. Then follow several chapters of results of observations and descriptions of the instruments used. Amongst these were a water pyrheliometer after Pouillet's model, a mercury pyrheliometer, two globe actinometers, a "solar comparator," and the spectro-bolometer. In chapter xiv. the amount of the atmospheric absorption is shown to be about 40 p. c., or double that usually supposed. Then follow chapters on "Sky Radiation," "Nocturnal Radiation," &c. Chapter xxi. gives a general summary of the results; and the most probable value of the "Solar Constant" is given as 3 calories. Three appendices follow: on the reduction of psychrometer observations, the determination of wave-lengths in the invisible prismatic spectrum, and also of the influence of convection currents on thermometer bulbs. The work is illustrated by twenty-one plates, mostly the observation curves of the different instruments, besides a map of the district of Mount Whitney, showing the proposed reservation, and a view of the mountain camp.

The principal results of Prof. Langley's work may perhaps be briefly summarized as follows:—He has shown that the invisible spectrum beyond the red extends much further than was imagined or believed possible; he has detected cold spaces in this spectrum analogous to the dark absorption bands and lines of the visible spectrum, and has determined their wave-lengths. He has shown that all the old formulæ for the determination of wave-lengths in the prismatic spectrum failed utterly when carried into the infra-red region. The amount of absorption exercised by the atmosphere has been proved to have been greatly under estimated; it is at least double what has been usually supposed, and the value of the "solar constant" has been increased in consequence. Contrary to the theory hitherto universally held, Prof. Langley has shown that the infra-red rays do not suffer the most absorption, but the visible rays and especially those in the green and blue, the transmissibility steadily increasing towards the extreme infra-red. From this it follows that what we call white light is not "the sum of all radiations," but that remainder of rays which has been filtered down to us, and the Sun, could we see it as it is, would appear of a decided bluish tint. Prof. Langley's observations seem also to indicate that at a certain point far in the infra-red, transmissibility through the atmosphere suddenly ceases, and it would seem to follow that the earth heat which fails to be radiated away through the atmosphere must be lower than we have yet examined in any spectrum. The old idea of a "temperature of space" is rejected, and reasons are given for concluding that the heat we receive from celestial bodies other than the Sun is practically *nil*. Finally, Prof. Langley concludes that the temperature of the earth under direct sunshine, even though our atmosphere were present as now, would probably fall to − 200° C. if that atmosphere did not possess the quality of selective absorption. To this catalogue of results it may be added that the volume before us gives abundant evidence of the attention and care which Prof. Langley has given to free his work from every source of error.

TUTTLE'S COMET.—Tuttle's Comet was found by MM. Perrotin and Charlois at the Nice Observatory on Aug. 8. The following observations were made by M. Charlois:—

	Nice M.T.			R.A.			N.P.D.		
	h	m	s	h	m	s	°	′	″
Aug. 8	15	39	25	7	20	25·21	61	22	31·0
9	15	11	9	7	23	43·15	61	58	35·7
10	15	11	51	7	27	4·02	62	36	1·3
11	15	28	39	7	30	26·73			
12	15	12	31	7	33	43·81	63	52	26·8
13	15	9	51	7	37	0·58	64	31	16·8

THE CORDOBA ZONE-CATALOGUE [*].—This Catalogue was printed from stereotype plates in order that any errors discovered up to the last moment might be corrected before the work should be actually printed. Notwithstanding this precaution, a considerable number of errors have been detected since the catalogue was issued. The Argentine Government has now directed Dr. Gould to cause the recently detected errors to be corrected upon the stereotype plates, and to offer these to the 'Astronomische Gesellschaft,' it being understood that the gift of the plates carries with it the fullest authority for the 'Astronomische Gesellschaft' to use them for printing a new edition whenever it may see fit.

NEW MINOR PLANET.—A minor planet (No. 249) of the 12th mag. was discovered by Dr. Peters on Aug. 16.

CINCINNATI OBSERVATORY.—No. 8 of the publications of the Cincinnati Observatory contains the Observations of Comets *a* and *b*, 1883, made by Mr. H. C. Wilson during his temporary Directorship. The observations were made with an opera-glass, a 2½-inch finder, and the 11-inch equatoreal. Besides observations of position there are copious notes on the appearance and 39 drawings of Comet *b* 1883, on 18 nights from 1883 Oct. 30 to 1884 Jan. 26. A discussion of the observations of the tail discussed according to Prof. Bredichin's method gives a value of $1-\mu$ a little less than 2·5, which agrees with that found by Prof. Bredichin from a large number of observations by different observers, the investigations being entirely independent, as Mr. Wilson's was finished before the receipt of Prof. Bredichin's. The present number is, like No. 7, a valuable addition to the literature of cometary astronomy.

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[*] Vierteljahrsschrift der Astron. Gesellschaft, 20 Jahrgang, S. 2.

YALE COLLEGE OBSERVATORY.—In the 'Observatory,' No. 90, p. 306, reference was made to Dr. Elkin's work with the Heliometer of Yale College. Various improvements have since been made in the instrument, and a registering micrometer added, which greatly facilitates the observer's work. In addition to measuring the distance and position-angle (from Alcyone) of those stars of the Pleiades which were measured by Bessel, several other stars have been measured. The diameters of Venus and Saturn, the satellite Titan, and the distance of lunar craters from stars have also been measured. It is intended to proceed now with the determinations of stellar parallax.

NATAL OBSERVATORY.—The work of this observatory is increasing so much that Mr. Neison thinks it highly desirable that two permanent assistants should be engaged. The meteorological observations introduced in January 1884 are now in full operation, and it is hoped that very soon, through the cooperation of several observing stations, a system of weather-warnings may be inaugurated. As regards the lunar work, we notice that 59 transits of the Moon's limb and 50 transits of the nearly central crater Murchison A have been obtained with the transit-circle. These observations furnish data for calculating the variation in the irradiation at edge of Moon, and of the effect of the known irregularities on the limb. The total now exceeds 100, and they are completely reduced. Arkley Observatory, England, cooperates in this work. An appendix to the report contains observations of comets Pons and Barnard. A good mean-time clock is wanted to automatically send time-signals to various parts of the colony.

STONYHURST COLLEGE OBSERVATORY.—The published results of this Observatory show that the magnetic and meteorological observations made in 1884 have been on the same plan as those of the past fifteen years. Numerous tables of monthly and yearly means of the usual elements are given. Father Perry remarks on a fall of rain noted on April 26—"In forty minutes 0·114 in. of rain was collected in our gauges. This rain was almost as black as ink and full of fine carbon in suspension." A comparison of the declination magnetograms of Kew and Stonyhurst now being made by Dr. Balfour Stewart promises some interesting results. On 257 days drawings of the Sun, 10½-inch to diameter, were made, the entire chromosphere was measured on 88 days, and spectra of spots obtained on 36 days. To enable solar observers to fill up breaks in the continuity of their series a list of Sun drawings during the years 1880–1884 has been inserted on the plan adopted by the Solar Physics Committee. We extract the following remarks on the upper glows in 1884:—

"The glow encircling the Sun during the day has never been entirely absent, though it varied in intensity from time to time, and was once or twice hardly perceptible. Sometimes it was merely a bright silvery glow without any warmth of tint, but more commonly the pink or salmon colour extended from the Sun to a distance of 18° or 20°. . . . The edges of the clouds in vicinity of the Sun have frequently been seen tinted with the colours of the spectrum. A glow exactly similar to the day-glow round the Sun has been seen encircling the Moon on Aug. 4, Sept. 1, 4, 5, 6, 26, Oct. 1, Nov. 3, 7, and Dec. 3, 4, and this late at night many hours after sunset." Observations of auroræ are given for Feb. 24, March 21, April 24, Sept. 17, 18, Oct. 3, 4, 16; and Father Perry remarks :—"Excluding the doubtful appearances of October, only five auroræ were observed in 1884, and they are all coincident with a disturbed condition of the solar surface. The strong polar shine of Sept. 17 was coincident with the largest spot of the year, which appeared first on Sept. 6, attained its maximum on Sept. 14, and was followed through various fluctuations in size till Dec. 6." The magnets were disturbed on Sept. 17. The remarks above would seem to strengthen what was said last year, that there is some evidence to show that the auroræ and magnetic storms synchronize rather with particular classes of spots than with solar disturbances generally." The zodiacal light was observed on March 18 and Nov. 14. Appended to the "Results" is the Report of the meteorological observations taken at St. Ignatius College, Malta.

THE MASS OF URANUS.—From his observations of Oberon and Titania, made during the oppositions of Uranus from 1881 to 1884, Prof. A. Hall finds the mass of the planet $=\frac{1}{22,682 \pm 27}$, rather less than the received value.

DETERMINATION OF THE POSITIONS OF CLOSE POLAR STARS [*].—M. Loewy has devised a method of determining the R.A.'s and N.P.D.'s of close polar stars, with great accuracy, by taking pairs of observations separated by an interval of four hours, the first observation being made two hours before meridian passage and the second two hours after. By this method the effect of the instrumental errors on the deduced positions is eliminated, and it is therefore unnecessary, for the success of the scheme, that these should be known. As an illustration of the accuracy that may be obtained in this way, M. Loewy gives the concluded R.A. and declination of Polaris (*sub polo*) on June 3, 4, and 5, compared with the places as given in the 'Connaissance des Temps,' the differences (O−C) for the three days being in R.A. $-0^{s}\cdot4$, $+2^{s}\cdot0$, and $-1^{s}\cdot5$, and in declination $+0''\cdot1$, $+0''\cdot8$, and $+2''\cdot1$ on each day respectively. M. Loewy proposes to publish a detailed memoir on the subject, to which we hope to refer on a future occasion.

THE CONSTANT OF NUTATION [†].—In his recent determination of the Constant of Aberration ('Observatory,' Vol. vi. p. 365) Herr

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[*] 'Comptes Rendus,' Tome ci. Nos. 1, 2.

[†] Astron. Nachrichten, No. 2667.

Nyrén discussed a series of observations of the three stars α Ursæ Minoris, Cephei 51 Hev., and δ Ursæ Minoris, which had been made by Herr Wagner during the years 1861–1872 with the transit instrument of the Pulkowa Observatory. Although the period embraced by this series of observations is little more than half that of the Nutation, still, considering the very great accuracy of these determinations, it has occurred to Dr. L. de Ball that it would be worth while to discuss them afresh with the view of determining from them a value of the Constant of Nutation. This he has accordingly done, with every attention to details, and has obtained the following results and mean errors:—

Correction to Peters' Constant of Nutation = −0″·006 ± 0″·012
Correction to Struve's Constant of Aberration = +0 ·043 ± 0 ·011
Parallax of α Ursæ Minoris .... = +0 ·015 ± 0 ·015
„ Cephei 51 Hev. .... = +0 ·027 ± 0 ·019
„ δ Ursæ Minoris .... = +0 ·034 ± 0 ·017
R.A. of α Ursæ Minoris (Eye and Ear) = 1^{h} 9^{m} 38^{s}·706 ± 0^{s}·043
„ „ „ (Chronographic) = 39 ·056 ± 0 ·051
„ Cephei 51 Hev. (Eye and Ear) = 6 36 11 ·723 ± 0 ·026
„ „ „ (Chronographic) = 12 ·012 ± 0 ·027
„ δ Ursæ Minoris (Eye and Ear) = 18 15 53 ·084 ± 0 ·020
„ „ „ (Chronographic) = 53 ·363 ± 0 ·020

These positions are referred to the Equinox of 1865·0.

The Cambridge Observatory.—Prof. Adams has published his Report for the year ending May 26, 1885. From it we learn that the total number of observations made with the transit-circle during the year was 3253, including 2442 observations of zone-stars, made on 100 nights. The observations of clock-stars and those of Polaris are completely reduced, and the mean places for Jan. 1 obtained up to the end of 1884. The true apparent places of all other stars observed in 1884 are also obtained both in R.A. and N.P.D. The meteorological observations are communicated daily by telegraph to the Meteorological Office. The sunshine recorder has been regularly employed, and the records sent at intervals to the office.

Dr. L. Struve, of the Observatory, Leipzig, wishes observers who made observations of Comet 1884 III. (Wolf), and have not published them, either to publish now or make them known to him, as he is about to work out its orbit.

A similar request for observations of Encke's Comet at its last appearance is made by Dr. Backlund of Pulkowa.

Mr. C. Todd, Director of the Adelaide Observatory, has requested us to insert the following paragraph, which he would have wished to have introduced into his speech at the June meeting of the Royal Astronomical Society:—

"Another very important work in which I have been engaged, and to which I may refer, is the recent determination of Australian longitudes. In conjunction with Mr. Ellery and Mr. Russell we made very careful determination of the difference of longitude between Singapore, Banjoemanjie, Port Darwin, Adelaide, and Melbourne, the results of which have been presented to our respective Governments in a joint report. Captain Darwin kindly undertook the work at Singapore, and at my instance the Government of the Netherlands despatched Capt. Helb to Banjoemanjie, Mr. Barrachi was sent to Port Darwin."

The 'Graphic' of Saturday, August 8, contains an excellent summary of the work done by the successive "Astronomers Royal." The paper is accompanied by a series of carefully executed drawings of Greenwich Observatory.

Dr. E. Lamp has recently investigated the annual parallax of Σ 2398 (D.M. 2164), and in the 'Astronomische Nachrichten,' No. 2676, gives the value he has obtained for it as $\pi = 0''\cdot34 \pm 0''\cdot034$.

Maxima and Minima of Variable Stars in October 1885.

M, signifies maximum; *m*, minimum.

Oct.	4	L^2 Puppis, *m*.
	5·3	T Monocerotis, M.
	9	U Canis Minoris, M.
	10	R Lyræ, M.
	10	T Herculis, M.
	11	S Libræ, *m*.
	12	R Vulpeculæ, M.
	15	R Sagittarii, M.
	17	S Aquilæ, *m*.
	18·3	ζ Geminorum, *m*.
	20	S Delphini, M.
Oct.	20	S Scorpii, M.
	20	R Corvi, M.
	21	R Coronæ, M.
	23·4	ζ Geminorum, M.
	24	S Pegasi, M.
	24	T Aquarii, M.
	24·4	T Monocerotis, *m*.
	26	R Aquilæ, M.
	27	S Vulpeculæ, *m*.
	28·5	ζ Geminorum, *m*.

Variables of Short Period.

δ Cephei.	d.
Oct.	1·55, *m*.
	8·55, M.
	17·65, *m*.
	19·25, M.
	24·65, M.
	28·35, *m*.

U Sagittarii.	d.
Oct.	1·5, M.
	5·3, *m*.
	21·7, M.
	25·5, *m*.
	28·5, M.

W Sagittarii.	d.
Oct.	2·4, *m*.
	5·5, M.
	17·6, *m*.
	20·7, M.
	28·3, M.

X Sagittarii.	d.
Oct.	6·3, *m*.
	13·3, *m*.
	20·3, *m*.
	27·3, *m*.

λ Tauri, *m*.		h	m
Oct.	30	17	11

η Aquilæ.	d.
Oct.	10·4, M.
	17·6, M.
	22·4, *m*.
	24·7, M.
	29·5, *m*.

U Cephei, *m*.		h	m
Oct.	2	7	24
	7	7	3
	24	17	51
	29	17	30

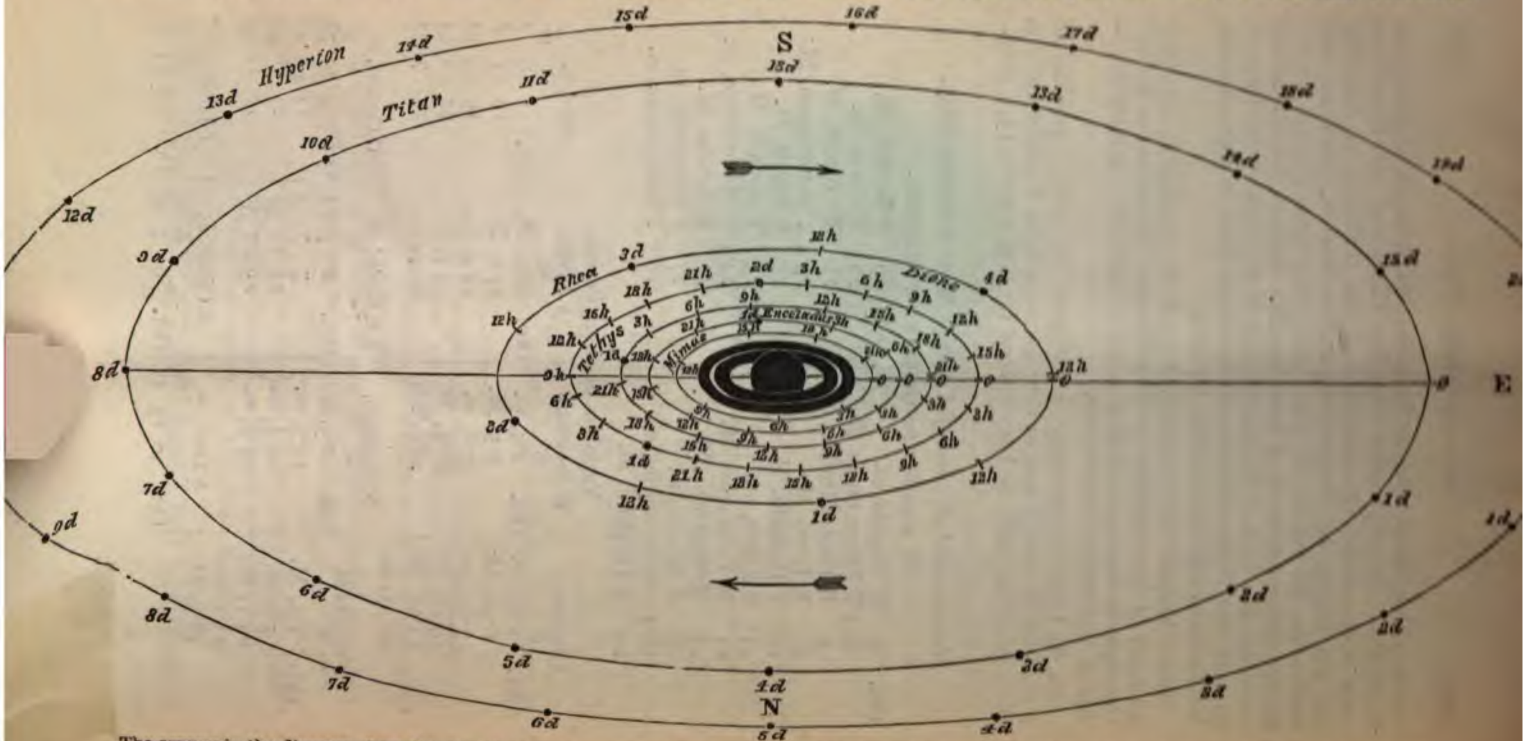
S Cancri, *m*.		h	m
Oct.	6	7	28

Algol, *m*.		h	m
Oct.	2	9	2
	16	17	6
	19	13	53
	22	10	44
	25	7	33

U Coronæ, *m*.		h	m
Oct.	4	10	38
	11	8	20
	21	16	54
	28	14	37

δ Libræ, *m*.		h	m
Oct.	6	11	51

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East elongation.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can usually be only seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS (East)	h
Sept. 25	16·9
26	15·6
27	14·3
28	13·0
29	11·6
Oct. 12	16·2
13	14·8
14	13·5
15	12·1
16	10·7
27	17·9
29	15·1
30	13·7
31	12·3

MIMAS (West).	
Oct. 4	16·0
5	14·6
6	13·3
7	11·9
8	10·6
20	16·4
21	15·0

ENCELADUS.	h
Sept. 25	12·0
26	20·9
28	5·8
29	14·7
30	23·6
Oct. 2	8·5
3	17·4
5	2·3
6	11·2
7	20·0
9	4·9
10	13·8
11	22·7
13	7·6
14	16·5
16	1·4
17	10·3
18	19·2
20	4·0
21	12·9
22	21·8
24	6·7
25	15·6
27	0·5
28	9·4
29	18·3
31	3·2

TETHYS.	h
Sept. 25	17·6
27	14·9
29	12·3
Oct. 1	9·6
3	6·9
5	4·2
7	1·5
8	22·8
10	20·1
12	17·4
14	14·7
16	12·0
18	9·3
20	6·6
22	3·9
24	1·2
25	22·4
27	19·7
29	17·0
31	14·3

TITAN.	
Oct. 3	0·0
18	23·0

HYPERION.	
Sept. 24	4·6
Oct. 15	12·5

DIONE.	h
Sept. 26	16·7
29	10·4
Oct. 2	4·1
4	21·8
7	15·5
10	9·2
13	2·9
15	20·6
18	14·3
21	8·0
24	1·6
26	19·3
29	13·0

RHEA.	
Sept. 27	11·7
Oct. 2	0·1
6	12·6
11	1·0
15	13·4
20	1·9
24	14·3
29	2·7

Apparent Elements of the Ring.

Greenwich Noon.	Position-angle of Minor Axis.	Latitude above plane of Ring. Earth.	Sun.
1885, Sept. 25	353° 9′	25° 29′ S.	26° 26′ S.
Oct. 10	353 8	25 23 S.	26 23 S.
25	353 7	25 22 S.	26 20 S.

Greenwich Noon.	Outer Ring. Maj. Axis.	Min. Axis.	Inner Ring. Maj. Axis.	Min. Axis.	Semi-maj. axis of Orb. of Titan.
1885, Sept. 25	41″·39	17″·81	27″·53	11″·84	186″·2
Oct. 10	42 ·55	18 ·25	28 ·40	12 ·14	191 ·4
25	43 ·73	18 ·74	29 ·08	12 ·46	196 ·7

The semi-major axis of the orbit of Titan is given as well as the position-angle of the minor axis of the ring, in order to furnish means for roughly inferring the distances and position-angles of the satellites from the diagram given on p. 316.

Differences of Right of Ascension and Declination between Iapetus and the Centre of Saturn. By A. Marth *.

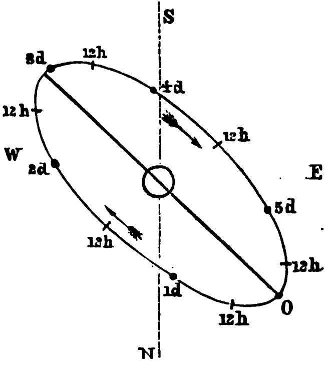
Iapetus—Saturn.

Greenwich Noon.		α−A.	δ−D.
		s	″
Sept.	25	+39·6	+ 86
	26	39·7	95
	27	39·6	103
	28	39·3	111
	29	38·7	118
	30	37·9	125
Oct.	1	36·9	130
	2	35·7	135
	3	34·3	140
	4	32·7	143
	5	30·8	146
	6	28·8	148
	7	26·6	149
	8	24·2	149
	9	21·7	148
	10	19·0	147
	11	16·2	144
	12	13·3	141
	13	+10·3	+136
Oct.	14	+ 7·3	+131
	15	4·1	125
	16	+ 0·9	118
	17	− 2·3	111
	18	5·5	102
	19	8·6	93
	20	11·8	83
	21	14·8	73
	22	17·8	62
	23	20·7	51
	24	23·4	39
	25	26·0	27
	26	28·4	15
	27	30·7	+ 3
	28	32·7	− 10
	29	34·5	22
	30	36·1	35
	31	−37·5	− 47

Satellite of Neptune.

Greenwich Mean Times of the North-east Elongations.

Sept.	5	2h·4
	10	23 ·5
	16	20 ·6
Sept.	22	17h·7
	28	14 ·7
Oct.	4	11 ·8
Oct.	10	8h·9
	16	6 ·0
	22	3 ·1
	28	0 ·2



Apparent Orbit of the Satellite of Neptune, as seen in an Inverting Telescope.

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* Monthly Notices, Vol. xlv. No. 8.

Position-angle of minor axis of apparent orbit on Oct. 1 = 322° 1′.

Semi-major axis of orbit on Oct. 1 = 16″·80.

The arrows in the diagram show the direction of the motion of the Satellite. The figures indicate the interval from the time of the last North-east Elongation. The central circle represents the planet, and is on the same scale as the orbit.

Astronomical Memoranda, October 1885.

Equation of Time:—Sun *before* Clock, Oct. 1, 10^{m} 27^{s}; Oct. 11, 13^{m} 18^{s}; Oct. 21, 15^{m} 21^{s}; Oct. 31, 16^{m} 18^{s}.

Sidereal Time at Mean Noon:—Oct. 1, 12^{h} 41^{m} 32^{s}; Oct. 11, 13^{h} 20^{m} 57^{s}; Oct. 21, 14^{h} 0^{m} 23^{s}; Oct. 31, 14^{h} 39^{m} 48^{s}.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
Oct. 3	18 7	5 35	26 21	+6 29	305 59
8	18 16	5 22	26 30	6 11	240 1
13	18 24	5 11	26 27	5 50	174 3
18	18 31	5 0	26 13	5 26	108 7
23	18 40	4 50	25 48	5 0	42 11
28	18 50	4 39	25 11	+4 32	336 14

The Moon.

	rises.			sets.			sets.			rises.	
	h	m		h	m		h	m		h	m
Oct. 1	10	47	Oct. 9	6	7	Oct. 17	12	15	Oct. 25	6	8
2	11	56	10	6	38	18	13	17	26	6	50
3	13	9	11	7	12	19	14	22	27	7	41
4	14	24	12	7	52	20	15	28	28	8	40
5	15	40	13	8	35	21	16	37	29	9	46
6	16	56	14	9	23	22	17	48	30	10	58
7	*sets*		15	10	17	23	*rises*		31	12	11
8	5	38	16	11	14	24	5	31			

Oct. 7^{d} 19^{h} 31^{m} New Moon. Oct. 15^{d} 13^{h} 21^{m} First Quarter.
23 9 23 Full Moon. 30 5 58 Last Quarter.

Occultations for October 1885.

Day.	Star.	Mag.	Disap.	Angle.	Reap.	Angle.
			h m	°	h m	°
1	λ Geminorum	3½	12 38	70	13 32	289
24	B.A.C. 987	6½	15 0	110	16 10	277
27	B.A.C. 1930	6½	12 0	89	13 6	281
29	1 Cancri	6	10 5	152	10 26	201

Planets for October 1885.

Day.	Planet.	R.A.			Dec.		Diam.	Rises.		Tr.		Sets.	
		h	m	s	°	′	″	h	m	h	m	h	m
1	Mercury..	11	52	19	2	54 N.	5·4	16	58	23	13	5	30
11		12	56	26	4	36 S.	4·8	18	2	23	38	5	16
21		13	58	22	11	47 S.	4·6	19	0	24	0	5	2
31		14	59	27	17	52 S.	4·8	19	55	0	20	4	48
1	Venus ..	14	56	4	17	52 S.	14·3	21	49	2	15	6	44
11		15	44	8	21	26 S.	15·1	22	20	2	23	6	29
21		16	33	49	24	6 S.	16·1	22	47	2	34	6	22
31		17	24	34	25	43 S.	17·4	23	10	2	45	6	21
1	Mars	8	46	15	19	12 N.	6·0	12	16	20	3	3	51
11		9	10	4	17	43 N.	6·2	12	9	19	48	3	27
21		9	32	47	16	7 N.	6·6	12	2	19	31	3	0
31		9	54	22	14	28 N.	6·9	11	54	19	13	2	33
1	Jupiter ..	11	29	22	4	26 N.	29·0	16	19	22	45	5	14
11		11	37	5	3	38 N.	29·2	15	52	22	13	4	39
21		11	44	32	2	51 N.	29·4	15	23	21	41	4	3
31		11	51	39	2	6 N.	30·0	14	55	21	9	3	29
1	Saturn ..	6	34	53	22	19 N.	16·8	9	42	17	50	2	2
31		6	35	51	22	18 N.	17·6	7	44	15	53	0	6
1	Uranus ..	12	14	32	0	49 S.	3·5	17	29	23	29	5	33
31		12	21	10	1	33 S.		15	42	21	38	3	38
1	Neptune..	3	33	35	17	22 N.		7	14	14	50	22	26
31		3	30	50	17	12 N.		5	14	12	49	20	24

Oct. 4^d 14^h. Mercury in conjunction with Uranus.
15 22 Mercury in superior conjunction.
20 1 Saturn stationary.

Phenomena of Jupiter's Satellites, October 1885.

d	h	m		d	h	m		d	h	m	
8	16	11	ii. Tr. E.	20	16	32	i. Tr. I.	28	18	0	iv. Oc. D.
11	16	37	iv. Ec. D.	21	16	10	i. Oc. R.	28	18	10	i. Oc. R.
12	16	51	i. Ec. D.	24	16	3	ii. Oc. R.	29	15	19	i. Tr. E.
13	16	50	i. Tr. E.	27	18	32	i. Tr. I.				
15	16	7	ii. Tr. I.	28	15	7	i. Ec. D.				

The R.A. and N.P.D. of Planets and the Solar Co-ordinates are given for Greenwich Mean Noon; in all cases Greenwich Mean Time is used, reckoning from noon to noon, as in previous years; the angles for Occultations are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 102. OCTOBER. 1885.

The New Star in the Great Nebula in Andromeda.

Of all the thousands of nebulæ now known to astronomers, the Great Nebula in Andromeda is the only one the discovery of which preceded the invention of the telescope. It was also the first upon which the telescope was turned, its telescopic history reaching back to Dec. 15, 1612, and it has therefore now been a subject of observation for nearly three hundred years. Yet, as a recent astronomical writer has remarked, "we are hardly more advanced than we were two centuries ago as to the explanation of this immense nebula. Whilst amongst those which have been discovered since, some have been resolved into clusters of stars, and others have proved their chemical constitution to be of a gaseous nature, this has remained silent and mysterious. Its spectrum is continuous, without transversal lines, and consequently the substances which compose it remain unknown. The highest powers have shown some 1500 stars in it, but it is not certain that these stars belong to it; they may simply be before it. Whilst its shape alters strangely according to the power employed."

This strange and beautiful object has broken silence at last, though its utterance may be difficult to interpret, and the most striking astronomical event of the present year, hitherto at least, has been the sudden appearance of a new bright star close to its nucleus. For the public announcement of this discovery we are indebted to Dr. Hartwig, now of Dorpat, who remarked the change in the nebula on Aug. 31, and at once telegraphed to Prof. Krueger at Kiel, who in his turn forwarded the intelligence to Lord Crawford, who, by one of his invaluable "Dun-Echt Circulars," apprised the English astronomical world on the morning of Sept. 2 of what had taken place. But before the intelligence had become widely distributed, a number of other observers had each independently discovered the stranger star, no fewer than nine being Englishmen. The majority of these observations, which speak so well for the close and careful attention our amateur astronomers keep on the heavens, were made on Sept. 1, 2, or 3. The discovery has naturally brought out the record of observations of the nebula made in the early part of August, and it is gratifying to observe that we have a nearly continuous record from the beginning of August up to the 18th, when Mr. Silcock examined the nebula. All these observations agree in declaring that no star was visible then, though many observers were much struck by the brightness of the nebula. But Mr. Isaac W. Ward reports that at 11 P.M., on Aug. 19, the star was distinctly visible; it had therefore assumed its new brightness not many hours before. On Aug. 22 the Baroness de Podmaniczky, in company with Dr. de Kövesligethy, remarked the new star, thus confirming Mr. Ward's observation.

The date of the outburst is thus fixed for us within very narrow limits; the notes, however, of several observers (we may refer to the remarks of Messrs. Tarrant, Saxby, and Prince) seem to indicate that for several weeks before the star became visible the nebula was showing an unusually bright appearance, so that the change was to some extent a gradual one. The observation of the Baroness de Podmaniczky, who found the star by no means a conspicuous object on Aug. 22, when considered in connection with the fact that so many observers discovered the *Nova* independently of each other about ten days later, would seem to suggest that it had increased in brightness in the mean time, and that probably it was at about its greatest brilliancy on Aug. 31 or Sept. 1. Estimates of its magnitude vary somewhat; but there seems strong evidence that the star has faded slowly, but steadily, since Sept. 1. Baron von Engelhardt determined the magnitude of the *Nova* as 6·5 on Sept. 1, 7·3 on Sept. 2, and 7·2 on Sept. 3. Prof. Pritchard found it 8·25 on Sept. 7, and 8·48 on Sept. 9. These were photometric determinations with the wedge. Mr. Knobel, by the method of limiting apertures, determined it as being 8·72 mag. on Sept. 3, 0·4 mag. fainter on Sept. 4, and half a magnitude fainter still on Sept. 9. Mr. Knobel adds the important remark that the brightest portion of the nebula is extinguished at the same aperture as it was in his observations recorded in the 'Monthly Notices' for March 1881. My own estimates, made simply by comparisons with the neighbouring small stars, and adopting Argelander's estimates of magnitude, were—Sept. 4, 8·0; Sept. 11, 8·3; Sept. 18, 9·2.

The accounts of the appearance of the new star have also not proved perfectly accordant; but, on the whole, there is a general consent that under low powers, at all events, it is purely stellar in appearance and of a decided orange-yellow tint; it has been compared to Arcturus or Aldebaran as to colour. With higher powers, however, several observers, Messrs. Common and Gledhill in particular, find that the new star shows a perceptible disk, Mr. Common comparing it to a satellite of Jupiter. Mr. Knobel, however, reports it as distinctly star-like with all powers up to 500, but both he and Prof. Pritchard notice that it goes out somewhat peculiarly when extinguished by their respective photometers. Mr. Tarrant records that on Sept. 4 the new star "was very hazy, not showing a clean disk with any power." So far as yet known, this observation has not been confirmed; if established, it would show that the star was then still in a condition of active change.

The spectrum, partly owing to the comparative faintness of the object, but chiefly to the character of its immediate surroundings, has proved rather a difficult matter for observation, and the various reports upon it differ considerably. It is clearly continuous, and though Prof. Vogel speaks of the red and yellow being bright and the green faint on Sept. 1 and 2, the English observations made a day or two later agree in representing from D to about *b* as very markedly brighter than the rest of the spectrum. Dr. Huggins suspected bright lines on Sept. 3, the existence of which he was able to confirm on Sept. 9. I was unable to detect any lines, bright or dark, on Sept. 4, but noted two slight irregularities in the spectrum, which were probably bright lines, on Sept. 11. Of these the more distinct was very near if not coincident with 1474 K, the other lay nearer D. It seems probable therefore that the star has a double spectrum, like the "temporary" stars T Coronæ, and Nova Cygni, 1876, and the variable γ Cassiopeiæ and others; *i. e.* a spectrum of bright lines superposed on a continuous spectrum, but that, as with Nova Cygni, the relative intensity of these has suffered alteration, and that the continuous spectrum is now fainter than at first, and therefore the bright lines are more readily seen. Prof. Vogel finds dark lines, not bright, one between the green and yellow, the other between F and G; and Dr. Konkoly regards the spectrum as one of the "fluted" type, such as we see in many red stars. The explanation of these discrepancies is to be found in the fact that the irregularities in the spectrum are but slight, and that the restrictions placed upon the use of a wide slit or of a cylindrical lens, through the star being seen on so bright a background, render their detection exceedingly difficult and uncertain.

Thus, though wholly unable myself to detect any dark lines in any part of the spectrum, the fact that the four points measured as bounding the entire spectrum, and its brighter central district, coincided closely with the four great lines, C, D, F, and G, suggests that they may possibly have been present, though not sufficiently distinct to be recognized as such.

The new star does not coincide with the nucleus of the nebula, though in the earlier observations it was generally assumed that it did do so, which is distinctly seen following it by a little over a second of time. Herr Spitaler, assistant at the Vienna Observatory, gives the apparent position of the *Nova* on Sept. 3 as $\alpha = 0^{h}\ 36^{m}\ 30^{s}{\cdot}73$; $\delta = +40^{\circ}\ 38'\ 26''{\cdot}9$; and its position relative to the nucleus as $1^{s}{\cdot}12$ *p* and $5''{\cdot}07$ *s*. There is a faint star in the nebula which has been frequently observed, and which precedes the nucleus by $10^{s}{\cdot}84$ according to Struvé and is $18''{\cdot}7$ to the *s*. This faint star Baron Engelhardt observed as $9^{s}{\cdot}42$ *p* and $15''{\cdot}80$ *s* of the *Nova* on Sept. 1. The *Nova* was announced to be in motion, but the report was due to seconds of time having been mistaken for seconds of arc.

Very diverse views have been expressed as to the connection of the star with the nebula. On the whole, the facts appear to favour the view that the connection is real and not merely optical. Not only have several observers, as already mentioned, noticed a brightening of the nebula previous to the appearance of the star, but several have believed that the present aspect of the nebula differs from its usual one. Mr. Ingall, for instance ('English Mechanic,' vol. xlii. p. 31), thinks that the nebula does not brighten so suddenly to the centre now as formerly, and Mr. Elger makes an equivalent remark. Other observers have noted that a dark rift has formed in the nebula close to the star. So far as the spectroscopic observations go, they support the same view; for though the spectrum of the star can be traced further than that of the nebula, this is mainly owing to its greater brilliancy, the extreme portions of the spectrum being very faint in both, whilst the brighter central portion of the one spectrum very closely corresponds to that of the other. The bright lines in the star spectrum are very difficult objects—and bright lines of the same relative intensity might possibly escape remark in the spectrum of the nebula.

There would be nothing unprecedented in the connection of a "temporary" star with a nebula. The well-known variable η Argûs is obviously associated with the Great Nebula in Argo, and, as Mr. Proctor remarks in a letter to the 'Times,' were the nebula ten times as far away as it is, we should have had the apparent formation of a new star when it blazed out in 1840. "Nova" Cygni, 1876, evidently was closely associated with a minute planetary nebula; Auwers and Pogson's remarkable observation of Messier 80, in 1860, furnishes a third instance of such a connection, and Hind's nebula in Taurus affords another somewhat parallel case.

It is premature, no doubt, to attempt to draw any very definite deductions from the present outburst; but it should always be clearly borne in mind that these "temporary" stars—and that this star may properly be classed as such is evident, for it is already fading—cannot be stars in the least resembling our own Sun in size or structure. Could the radiations of our Sun be suddenly increased a thousandfold, it would be absolutely impossible for it to resume its former scale of brightness within the course of a few weeks or months. The suddenness of the increase and subsequent decrease of light prove unmistakably that we have to do with bodies relatively minute, and therefore capable of rapid cooling. The only alternative—that the star has not really altered its lustre, but that some intervening screne has been temporarily withdrawn—seems inadmissible, especially in view of the spectroscopic history of T Coronæ and Nova Cygni. Since, then, the distance of these stars is so great (for as yet an appreciable parallax has not been obtained for any one of them) and their brilliancy so high, we are compelled to substitute for a compact Sun a loosely scattered, widely extending system of small bodies—a system similar in character, but on an indefinitely larger scale, to that which we see in the corona or in the rings of Saturn.

If this deduction be correct, we may suspect that the spectroscope in revealing to us the gaseous nature of so many nebulæ has not told us the entire truth, and that solid bodies, varying greatly in different nebulæ as to their numbers and size, are frequently important constituents of them. Probably nebulæ vary indefinitely in the relative proportions of the solid and gaseous matter in their composition, from those which are nearly purely gaseous to those which contain scarcely anything but solid bodies. If these were exceedingly numerous, so as to overpower the gaseous spectrum of the nebula by the continuous spectrum they gave, and yet none were sufficiently large to appear as a distinct star, we should have presented to us the very features we recognize in the Andromeda nebula, and Sir John Herschel's prevision, made more than half a century ago, would be realized, that "the great nebula in Andromeda may be, and not improbably is, optically nebulous owing to the *smallness* of its constituent stars."

The cause of the appearance of these "temporary" stars still remains to be found. The letter of Prof. Monck's, which we print on pp. 335 and 336, contains an ingenious suggestion. Certainly, if we may consider Mr. Denning's observation of fixed meteor-radiants as proving that meteor-streams moving with a velocity of 200 miles a second and upwards are features of interstellar space, the passage of such a stream through a nebula might not be a matter of rare occurrence, seeing how many pass through our own system; and granted that the stream was sufficiently rich, and covered a sufficiently extensive area, all the phenomena of a temporary star might well be produced.

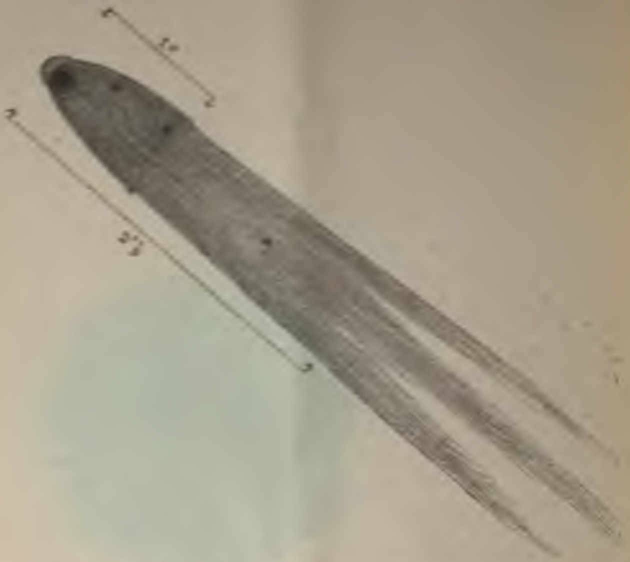
E. W. MAUNDER.

The Comet of 1812 (*Pons-Brooks*).

NOT having seen any observations of the peculiar triple appearance of the tail of the above comet on January 21, 1884, I have thought an observation and a sketch of the comet's appearance in the telescope that night might be of value.

On the night of January 20 the stellar appearance of the nucleus was very remarkable; it was small, clearly defined, and very bright. For a distance of probably one degree behind the head the body had the appearance of a brush of light, terminating at that distance rather abruptly; but from out of this there extended for a considerable distance further a much fainter tail, which with the naked eye could be traced for about 18°, running along parallel to the southern edge of the zodiacal light. The fainter part of the tail with the telescope occasionally exhibited numerous fine dark lanes parallel to its axis.

There was considerable coma about the nucleus; this seemed to extend beyond the outline of the head in front. A very faint envelope extended completely around the head. One degree behind the nucleus, near the extremity of the abruptly terminated body, there seemed to be a continual disturbance in the cometary light. The



southern border of the head was decidedly more curved than the northern side. The whole body of the comet (*i. e.* the head extending back to the abrupt termination) was very *flat* in appearance, the light being even all over, like that from a flat disk.

On January 21 the nucleus, which had appeared so distinctly stellar on the night previous, had melted away into a mass of dense haze, the star-like form having vanished. The faint envelope extending about the head on the 20th could not be seen, though it was closely looked for. Otherwise the general appearance of the head and body was the same; but there seemed to be a decided pulsation in the light of the comet at a point about 1° back from the nucleus; though the appearance was probably imaginary, the body of the comet seemed to rapidly swell and contract, so that a small star close to it would seemingly for a moment be involved in the nebulous light and the next instant free of it. Nevertheless, the definition was good and the phenomenon striking.

The greatest change had occurred in the tail, which had separated into three distinct branches. The spaces between these branches were entirely free of nebulous matter and decidedly black; especially was this noticeable between the central and southern branches; the southern branch was much the brightest, the northern being very faint. The separation that caused the northern branch commenced at a somewhat greater distance from the head than the other. At this observation the head and body still retained the disk-like surface, and the coma extended considerably in front and almost obliterated the outline of the head. The sketch was made at about $7^h\ 30^m$ Nashville M. T.

The observations were made with the 5-in. refractor and comet eyepiece of about $1\frac{1}{4}°$ field. E. E. Barnard.

Vanderbilt University Observatory,
Nashville, Tenn., U.S.A., 1885, Aug. 28.

Commensurability of Motions *.

It is well known to astronomers that if two planets or satellites, in motion round the same centre of attractive force, have their periodic times, or mean motions, nearly commensurable, there will exist in the motions of these bodies certain curious and interesting peculiarities. About two centuries ago it was observed that the motion of Jupiter was slowly accelerated, and that of Saturn retarded. These irregularities perplexed astronomers for a long time, but at last they were explained by Laplace. He pointed out the fact that since twice the period of Saturn is nearly equal to five times the period of Jupiter, the mutual attraction of these planets produces a large irregularity in their motions; and this irregularity itself has a period of about nine hundred years. Many other similar cases have been found in our solar system, though none where the irregularity is so large. The masses of Jupiter and Saturn are so great that the mutual perturbations are very large, and the problem of computing them is a difficult one. Laplace gave the first investigation, and was followed by Pontecoulant and others, and especially by Hansen in his elaborate prize memoir on this subject. Recently, Leverrier has investigated this question, but unfortunately his tables of Saturn are not much better than the old ones of Bouvard published in 1821.

But suppose the motions of two bodies should be exactly commensurable, what may we expect under such conditions, and would such a system be stable? This question is not directly answered in the 'Mécanique Céleste,' although it is safe, I think, to infer the opinion of Laplace that there is nothing unstable in such a system.

Still the belief that such a system would be unstable, and would be destroyed by the slightest disturbance, seems to prevail extensively, and even among some astronomers of repute. Thus gaps in the ring of asteroids and divisions in the ring of Saturn have been explained on the theory that it is impossible for bodies to move in such places, since their motions would be commensurable with those of certain planets or satellites. But such conclusions do not rest on any solid foundation, and seem to be drawn from the formulæ and expansions of the 'Mécanique Céleste' and their application to an extreme case. All such formulæ, however, are derived under certain assumptions, and we must be cautious how we undertake to limit the phenomena of the universe by our interpretation of them. Hansen has remarked that in the case of commensurable motions, the common formulæ must be modified to meet the new conditions. An interesting discussion of the case will be found in 'Hendrick's Analyst' for April 1874. This is by Mr. G. W. Hill, and he shows that commensurability of motions has no marked connection with the stability of a system. But since the invention of the telescope we have had an example before our eyes which should have prevented too hasty conclusions in this matter. It is that of the three inner satellites of Jupiter. So far as observations show, the motions of these satellites are exactly commensurable, and yet this system shows no sign of instability or decay. The consideration of such a system should have warned us against too general deductions from our formulæ.

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* From 'The Sidereal Messenger,' Sept. 1885, pp. 200-202.

It is worth while to notice, in this connection, a curious case brought forward by Laplace and which for a time caused much discussion. It had been the custom to teach that the Moon was made to give light to the Earth. But Laplace found a special case of the motion of these bodies which he announced as an improvement in the present arrangement. He says:—"If, at the origin, the Earth and the Moon had been placed on the same right line, at distances from the Sun proportional to one and one plus one one-hundredth; if, moreover, they had been given velocities parallel and proportional to these distances, the Moon would have been continually in opposition to the Sun; these two stars would have succeeded each other above the horizon, and as, at that distance, the Moon could not be eclipsed, its light during the night would have replaced the light of the Sun." This bold improvement of creation, by means of which we should always have a full Moon, was startling, and brought out criticism. In a Latin essay, published at Rome in 1825, a writer took the ground that on account of refraction and parallax the conditions proposed by Laplace would never exactly occur. But such criticisms are trivial and they produced no effect. It was reserved for the mathematician, Liouville, to show in 1842 that the system proposed by Laplace is essentially unstable, and could never exist in Nature. This point seems to have been overlooked by Laplace, since he repeats his assertion in the 'Système du Monde.' ASAPH HALL.

Souchon's 'Traité d'Astronomie Pratique'[*].

This treatise on practical astronomy is specially designed to explain the various processes and methods used in the construction of Astronomical Ephemerides as they are published at the present day, with special reference to the construction of the 'Connaissance des Temps.' Other authors have incidentally treated of these matters, but we believe that this is the only work recently published which is altogether devoted to this branch of practical astronomy, and as such deserves more than a passing notice from us.

The main body of the work is preceded by an Introduction treating of the history of Ephemerides and of Astronomical Tables, together with an historical notice of the principal phenomena, the prediction of which forms the subject of our Ephemerides. This Introduction is little more than a history of French Astronomy in general, and of the 'Connaissance des Temps' in particular—interesting in itself, but hardly calculated to convey to the reader an accurate impression of the relative importance of the various Astronomical Ephemerides and Tables which have been published. The account of Star Catalogues is singularly defective. The only Catalogue constructed during the last twenty years which the author mentions is M. Loewy's Catalogue of Moon Culminators published in the 'Annales du Bureau des Longitudes.'

Passing on to the more essential parts of the book we find in the First Part what M. Souchon calls the "Corrections des positions Célestes;" that is, the subjects generally treated of in works on Spherical Astronomy. We have here briefly and very intelligibly explained such subjects as Refraction (Laplace's methods only); Parallax, including the explanation of the corrections necessary on account of the spheroidal figure of the Earth; Precession and Nutation, and Aberration. With regard to the last named we may remark that no account of the subject can *now* be considered complete which does not explain the effect of the motion of translation of the Solar System; no reference to this, however, is made in the work before us. We also remark that the author gives $20''\cdot000$ as Bessel's value of the Constant of Aberration; as far as we know, Bessel has nowhere published as his such a value of the Constant[†]. In this Section, as in the following ones also, many of the notes are of great importance, especially to students of astronomy. They frequently explain very aptly the mathematical difficulties which from time to time are encountered.

The Second Part goes on to explain the construction of Ephemerides, and treats successively of the Calendar, Ephemerides of the Sun, Planets, and Moon, the determination of apparent positions of Stars, and the Calculation of Lunar Distances. These are explained ably and well, and give evidence that M. Souchon is complete master of his subjects, at all events as far as their application to the construction of the 'Connaissance des Temps' is concerned.

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[*] 'Traité d'Astronomie Pratique, comprenant l'exposition du Calcul des Ephémérides Astronomiques et Nautiques, d'après les méthodes en usage dans la Composition de la *Connaissance des Temps* et du *Nautical Almanac*, avec une Introduction Historique et de nombreuses notes,' par M. Abel Souchon. Paris: Gauthier-Villars, 1883. 8vo. Pp. 399.

[†] In the same note the name of the famous Bishop of Cloyne is printed "Brinckley."

The Third Part treats of the prediction of astronomical phenomena, including the phenomena of Jupiter's Satellites, Eclipses of the Sun and Moon, Occultations of stars and planets by the Moon, Tides, Transits of Venus and of Mercury, and disappearances of the ring of Saturn. These subjects are gone into very fully, and the application of the various formulæ illustrated by numerical examples. Special attention is paid to Solar eclipses, and Woolhouse's method, as developed in the "Additions" to the 'Nautical Almanac' for 1836, is carefully explained.

The Fourth Part contains a very valuable *résumé* of the formulæ contained in Parts II. and III. The Fifth Part consists of Tables to facilitate the calculation of Ephemerides, for the most part extracted from the 'Connaissance des Temps.' Those numbered xiii., xiv., and xv., referring to the calculation of occultations, are due to M. L. Schulhof.

The references to the 'Nautical Almanac' in M. Souchon's treatise are but few. It must therefore be looked upon as essentially a companion to the 'Connaissance des Temps'; and although this circumstance may to some extent interfere with its usefulness and popularity (outside France), we gladly acknowledge that the book is a very valuable one, and supplies what has long been a want in the literature of Astronomy. A. M. W. DOWNING.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The New Star in the Great Nebula of Andromeda (Messier 31).

WE have received the following communications respecting the above object:—

I. *Date of the Appearance of the Nova.*

JOSEPH GLEDHILL, F.R.A.S., Bermerside, Sept. 18, 1885.

I have never paid any particular attention to this nebula, but have often used it as a "show" object for visitors to the observatory.

On the 13th of last August two gentlemen visited the observatory about 10 P.M., and after showing them Vega and ϵ Lyræ and the cluster in Perseus, I turned to the nebula in Andromeda, and was at once struck by its brightness, and thought at first that I had got hold of a bright comet. As I was using a very low power on the $9\frac{1}{3}$-inch refractor I did not see the nature of the apparently increased brightness, *i. e.* whether it was stellar or merely a general increase. Bad weather followed, and I left Halifax on the 28th for a couple of weeks. On my return my attention was drawn to the wonderful changes in this beautiful nebula. Since my return the sky has been clear on but one night, viz. Sept. 13th, when I carefully examined the object, and of course, even with the same power (about 60), at once saw the bright nucleus or star in the nebula. With a higher power I thought it behaved like a planet.

B. J. Hopkins, F.R.A.S., Forest Gate, Sept. 19, 1885.

With reference to this object I should like to state that I can confirm the Brussels observer's statement that the *Nova* was not to be seen at the commencement of August; previous to the announcement of its discovery the last time I observed the nebula was August 10, when, as seen with a 3-inch refractor and powers of 50 and 100, *no trace* of the star was then visible.

Dr. N. de Konkoly, O'-Gyallai, Sept. 5, 1885.

Dr. Ralph de Kövesligethy, astronomer of my observatory, had spent the whole of August at my friend's, Baron G. de Podmaniczky, in East Hungary, where, in company with the Baroness, he observed with a comet-seeker of $3\frac{1}{2}$ inches aperture. In the first half of the month they several times saw the nebula under its ordinary aspect, the last occasion being August 13, when they showed it to Stefan de Tisza, eldest son of our Minister-President. They could not perceive the smallest change in it.

On August 22 and 23 the Baroness remarked to Dr. de Kövesligethy that she saw a little star in the nebula, and he could but confirm her observation. Yet the phenomenon was so faint that both believed it was caused by the full Moon, the brightness of which caused the fainter details of the nebula to fade away, so that only the central part remained. They were the more disposed to take this view, since they only had magnifying-powers of 12, 18, and 26 at their disposal.

The priority of the discovery of this new star belongs therefore to the Baroness de Podmaniczky, *née* Grāfin Bertha de Degenfeld, who determined its apparition as taking place between August 13 and 22; and surely Dr. Hartwig, being a gallant gentleman, will not dispute it.

C. L. Prince, F.R.A.S., Crowborough, Sept. 18, 1885.

An observation of mine upon the nebula of Andromeda on Aug. 15, and which I communicated to Dr. Copeland on the 4th inst., immediately upon receipt of the Dun-Echt Circular No. 97, is of importance in reference to the time of the first appearance of the *Nova*.

On the night of the above date, as I was sweeping the sky with a comet-eyepiece on my equatoreal, the Andromeda nebula passed across the field, and its brightness was such that I did not recognize it as the nebula, and thought that I had found a comet. I therefore looked at it for several seconds, and can state very positively that the *star* was *not* then visible.

Rev. S. H. Saxby, East Clevedon, Sept. 4, 1885.

On August 6th I was showing a few common objects to some friends at Davos with a 6-inch Calver mirror. I was myself so much impressed with the aspect of the great nebula in Andromeda that, the night of Aug. 9 being marvellously clear, I was induced to make it the chief study, ascribing the singular brightness to the purity of the air, the definition being perfect. The nebula was readily visible for 2° 45′ (measured) in length and for 30′ in breadth, and two long rifts were perceptible along the upper or following flank, though not so clearly but that they would probably have heen missed had I not been aware of their existence. The whole central portion of the nebula was strikingly bright, so much so that I turned to the nebula in Vulpecula to see whether the Alpine air was doing as much for it. Finding no difficulty in seeing the shoal of little stars projected upon it, not always easy objects, I took for granted that the unusual brightness was merely casual, and applied the spectroscope to see whether under such circumstances any lines could be made out. I could find none. The bright lines of the Vulpecula nebula had been plain enough, but here I got nothing more than the usual dull continuous spectrum, deficient at the red end. There was no sign whatever of a stellar nucleus, nor was there any thing which could so much as raise the question of the existence of one. The centre of condensation was purely nebulous, and the condensation was not abnormally rapid.

The following night, Aug. 10th, I examined it once more, both with eyepiece and spectroscope, in the hope of picking up a hydrogen-line. The results were, however, precisely as before; and I left Davos next day. On returning to England I unhappily went into Warwickshire for a few days before going home; and on getting to the telescope again at the earliest opportunity, the first thing I did, after focussing upon a double star, was to examine the Andromeda nebula as a comparative air-test. This was Sept. 3rd, at 9.30. Amazed at seeing a bright stellar nucleus, I hurried away to catch a passing mail-cart with a communication to Greenwich, and the next day brought the news that the change had been already observed at Dorpat.

P. H. Silcock, Brixton, Sept. 11, 1885.

On Aug. 17 and Aug. 18 I saw the Andromeda nebula with my 3½-inch Cooke; the only note then made refers to my difficulty in getting definition. For a fortnight I did not use the glass; and on Sept. 1st I noticed the new star. The appearance puzzled me considerably, and being but a beginner, I attributed it to some fault of my own. I was a good deal astonished on seeing Saturday's 'Times' to find that I was one of the first to see the star.

KENNETH J. TARRANT, Pinner, Sept. 9, 1885.

Whether the outbreak of the new star in the great nebula in Andromeda is connected in any way with a physical change in the constitution of the nebula itself is a most important question; and although the following observation may not throw much light on the subject, it seems to indicate the possibility of such a change having taken place. When observing the nebula under exceptionally favourable circumstances on July 2nd I was much surprised by the increased brightness of its central portion. With a power of 320 the appearance was exactly as though a considerable star were shining *behind* the nebula and illuminating the centre of it. The brightest part appeared to be of a lenticular shape, and, although suggesting the idea of a stellar nucleus, no power would show the smallest star-point in it. On July the 6th the centre was observed to be quite as bright, although the light was more diffused. Absence from home prevented any further observations: but on looking at the *Nova* on the 4th inst., I found on comparing a drawing made on the 2nd July its place was almost exactly in the centre of the then brightest portion. It appeared very ill-defined with any power over 120, and in colour was about equal to Arcturus. I came to the conclusion that, although the aspect of the nebula was unusual, there was not sufficient evidence of absolute change to warrant an announcement, as I not unnaturally thought the excessive transparency of the air might be answerable for it. Notice of it was, however, sent to two friends.

H. MICHELL WHITLEY, Westminster, Sept. 8, 1885.

I independently discovered the variation in the great nebula in Andromeda. I did not observe it between Aug. 9 and Sept. 3; on the former date it presented the appearance I have seen for the past fifteen years—"a dim haze condensed into a ball of milky light at the centre."

On turning the telescope on the nebula on the night of Sept. 3rd, I at once observed the change, the nucleus appearing as an intensely bright point resembling a 7th mag. yellow star.

Besides the preceding letters we have also received communications from Messrs. B. Kidd, Bramley, Guildford, and G. W. Middleton, Mexbro' Common, who each discovered the *Nova* independently—the former on Sept. 1, the latter on Sept. 3.

II. *Appearance, Brightness, and Spectrum of the Nova.*

W. HUGGINS, D.C.L., Upper Tulse Hill, Sept. 12, 1885.

The star was observed here first on the night of the 3rd instant. It presented the appearance of an orange-coloured star of from the 8th to the 9th magnitude. With a spectroscope of low dispersive power a continuous spectrum was seen from about C in the red to a little beyond F. There was an apparent condensation of light from about D to *b*, which might be due to bright lines in that part of the spectrum. This supposition was strengthened by the employment of a more powerful spectroscope, but I was not able to be certain on this point.

On the night of the 9th instant, the star, which was then seen distinctly on one side of the principal point of condensation in the nebula, appeared to me to have a less decided orange tint. It presented a similar appearance in the spectroscope, with the exception that the light was less strong about D. I was so far confirmed in my suspicion of bright lines, that I have little doubt that from three to five bright lines were present between D and *b*.

On the 3rd instant the star did not appear clearly defined in the refractor of 15 inches aperture; but the state of the sky was not good enough to enable me to be sure that the star was truly nebulous. On the 9th the star was certainly free from nebulosity.

B. Kidd, Bramley, Guildford, Sept. 8, 1885.

I have seen the *Nova* on six nights of the eight in September, and I believe it has diminished in brightness. On the 1st it was *conspicuous* in my 1½-in. finder. I can no longer see it as a *star* in the finder, although the nebula looks brighter than usual towards centre. I am not keen-sighted at all; others might still see it with 1½ in. finder; but I noted it as being less bright on 6th, when it was very clear here.

Dr. N. de Konkoly, O'-Gyallai, Sept. 5, 1885.

When on Sept. 4 I received the Dun-Echt Circular, No. 97, announcing change in the Andromeda nebula, I at once observed this object, which I had last seen on Aug. 9, when showing it to two professors of the Presburgh Royal School. I was greatly astonished to see now, even with the finder of my 10-inch Merz refractor, a bright star in the centre of the nebula, though I had seen none at all on the last occasion.

I observed the star with the 10-inch Merz refractor, with a Zöllner eyepiece spectroscope, and a neutral-tinted wedge.

The colour of the star is reddish yellow, its diameter is about 2″, and its disk is not sharply defined. The spectrum is very faint even without the cylindrical lens. It seems that there are three broad bright fields respectively in the red, yellow, and blue, and a fainter one in the green. If this be so, the spectrum would seem to show the hydrogen lines C, F, and D_3 under high pressure. But I rather regard it as a spectrum of the Type III. *b**, which, if the phenomenon should be substantiated, would offer a strong proof in favour of the theory of M. Ritter of Aachen. It is surely of some importance that the blue end is cut off close above F. Dr. Kövesligethy made observations of a precisely similar character.

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* [Vogel's nomenclature, Secchi's fourth type, the red stars, of which No. 152, Schjellerup, may be taken as a specimen.—Ed.]

After the spectroscopic observations of this interesting object, I made some photometric ones, together with Dr. de Kövesligethy, with two wedges. The one by Steinheil belongs to M. de Gothard, and is furnished with an ingenious registering apparatus, the second is made by Horne and Thornthwaite. At $10^h\ 10^m$ O′-Gyallai M.T. we compared the *Nova*, then at an altitude of 52°, with 1302 Lal., $0^h\ 38^m\ 3^s{\cdot}52 + 40^\circ\ 2'\ 24''{\cdot}1$ (1800), mag. 7·5. The wedges being new, we do not yet know their absorptive power, and must therefore delay the reductions.

E. W. Maunder, Greenwich, Sept. 22, 1885.

I have obtained the following spectroscopic observations of the new star with various spectroscopes on the 12·8-inch refractor of the Royal Observatory.

Sept. 4. Continuous spectrum, differing only from that of the nebula in that it is brighter and showing no lines, either bright or dark. Spectra both of star and nebula scarcely visible below D or above F. Half-prism spectroscope, one prism, direct and reversed.

Sept. 11. The following estimates were made of the extreme limits to which the spectra of the two stars Arg. Z + 38° Nos. 90 and 88, of the *Nova*, and of the nebula could be traced:—

Object.	Mag.	Red end.	Violet end.
No. 90	5·5	λ 7400	H
No. 88	8·0	6800	*h*
Nova	8·3	6600	G
Nebula	..	6200	Hydrogen

From this it appears that the spectrum of the *Nova* is shorter at both ends than that of an ordinary star of about the same magnitude, and that the nebula shows decidedly less red light than the star. Two bright lines were suspected, the brighter at λ 5480 ±, the fainter at λ 5330 ±. The latter might *possibly* be 1474 K. The part of the spectrum from D to F is very much brighter than the rest. The same district is also much the brightest in the spectrum of the nebula. Star-spectroscope. One prism of 60°.

Sept. 15. Observation of the bright lines repeated with an eyepiece prism.

C. L. Prince, F.R.A.S. Crowborough, Sept. 22, 1885.

On the night of Sept. 4 the nucleus was brighter than on the previous evening, and with a positive eyepiece, power 434, showed an apparent disk like a planet's satellite.

New Stars and Shooting-Stars.

Gentlemen,—

Probably the following hypothesis has been propounded before, but I am not aware of its author, and in relation to the new star in Andromeda it seems worth stating again. It is, in short, that as shooting-stars are known to be dark bodies rendered luminous for a short time by rushing through our atmosphere, new stars are dark (or faintly-luminous) bodies which acquire a short-lived brilliancy by rushing through some of the gaseous masses which exist in space.

The existence of luminous gaseous nebulæ in space has been rendered certain by the spectroscope. The existence of dark gaseous nebulæ and dark stars (if the phrases be not improper) is at all events highly probable. The vast extent of some of the known gaseous nebulæ renders it much more probable that a star should rush through one of them, than that two stars should come into collision. Gaseous nebulæ sometimes occupy degrees in the sky, even supposing that the whole nebula is sufficiently luminous to be visible to us, while the angular dimensions of a fixed star are always too small for measurement. The rush of a star through a gaseous nebula is therefore no improbable event; and it is certain that if it occurred the star would be highly heated, and its light greatly intensified. A star previously dark might be rendered luminous by this process, and a nebula previously dark might also be rendered luminous by the passage of the star, as our shooting-stars sometimes leave streaks of light behind them. The heated gas round the star might in such cases render the spectrum partly gaseous, but unless the heat was such as to vaporize the star it could not become wholly so. Now it would seem that new stars almost always occur in connection with nebulæ, and so does another phenomenon which appears to be of the same kind, viz., variability either wholly irregular or with a very long period. The variability of a star would of course be irregular if its orbit was such as to pass through a nebula once without intersecting it a second time; while it would probably have a long period if the orbit after leaving the nebula returned to it again. Not wishing to trespass too far on your space, I leave this hypothesis to be developed or refuted by others.

Yours truly,

Dublin, 1885, Sept. 18. W. H. S. Monck.

Remarkable Meteor-train.

Gentlemen,—

Shortly after eleven o'clock last night (Sept. 4th), when going upstairs to have a look through the telescope at my front window, I caught sight of what appeared to be the tail of a large comet at my back window. On looking (with the naked eye) it extended from a small cloud fringing the north-eastern horizon to the vicinity of the star β Aurigæ. It speedily became fainter and disappeared in perhaps two minutes from my first sight of it. From the part of the sky where it appeared I took it to be an auroral display, and watched for its re-appearance for ten minutes or a quarter of an hour until the sky became overcast. During this time I saw four meteors in the same region, but all of them at a greater altitude—one decidedly brighter than Capella. This evening, while watching for a longer time, I saw but one meteor. The cometary appearance of the meteor-train described by Mr. Brooks of Phelps, N. Y., in 'The Observatory' for September 1884, satisfied me as to the true character of what I had seen, more especially as other observers had seen the meteor of which Mr. Brooks only saw the train. I had never seen a meteor-train nearly so bright before, and it may have been fading when I saw it first. I should wish to know whether any of your readers observed the meteor in question or the shower of which it formed a part, and which in a clearer sky would probably have been the finest that has been witnessed for some years.

Truly yours,
W. H. S. MONCK.

13 Belvedere Place, Dublin.
1885, Sept. 5th.

A Bright Meteor.

GENTLEMEN,—

On Sunday evening, Sept. 13, at 6.46 P.M. (consequently in very strong twilight), I saw a large meteor fall from N. to N.E. So bright was it, that it attracted the attention of other passers by. Leaving a distinct train, it burst without sound approximately near Capella. I merely send you this note for what it is worth: it may interest meteoric observers, for I cannot recollect reading of any daylight observations of this kind, although I have no doubt of their existence. The meteor must have been of large size to be visible during such a bright light, and the time the train lasted (perhaps 3 seconds) rather substantiates that supposition. EDMUND J. SPITTA.

Ivy House, Clapham Common
1885, Sept. 21.

Changes in the Length of the Red Spot on Jupiter.

GENTLEMEN,—

Mr. Barnard's letter ('Observatory,' July 1885, p. 244) on this subject has induced me to compare a number of other results with those he has given. His estimates of the length of the red spot are considerably below those indicated by the observations of Hough, Kortazzi, Niesten, Pritchett, Williams, myself, and others. A careful analysis of several hundreds of observations has also shown that his opinion, as to the maximum length of the spot being attained early in Oct. 1880, is not confirmed. I have summarized the results into monthly mean values and have not extended them beyond January 1882, as very few observations have been made since then of the middle and *p.* and *f.* ends of the spot. I subjoin the results in three cases where the observations have been sufficiently numerous and extended for comparison.

*Niesten.**

	Length of spot.	Obs.
1879.	°	
Nov.	31·7	2
Dec.	33·1	6
1880.		
Jan.	33·5	2
Feb.	32·6	1
July	29·9	2
Aug.	32·6	2
Sept.	33·3	4
Dec.	34·7	1
1881.		
Aug.	35·3	1
Sept.	34·4	4
Oct.	31·1	1
1882.		
Jan.	32·6	1

Williams.

	Length of spot.	Obs.
1880.	°	
Aug.	31·4	2
Sept.	29·8	5
Oct.	30·6	3
Nov.	30·8	5
Dec.	26·9	2
1881.		
July	29·5	6
Sept.	33·6	5
Oct.	34·5	6
Nov.	36·2	4
Dec.	33·9	3
1882.		
Jan.	32·9	1

Denning.

	Length of spot.	Obs.
1880.	°	
Sept.	27·8	2
Oct.	33·7	6
Nov.	34·4	10
Dec.	33·9	6
1881.		
Jan.	33·4	4
July	31·0	3
Aug.	34·1	4
Sept.	32·7	7
Oct.	32·6	1
Nov.	32·9	2
Dec.	32·7	7
1882.		
Jan.	32·6	4

Prof. Hough's measures with the 18½-inch refractor of the Dearborn Observatory are very valuable in reference to this question:—

Date.	Length of spot.	Obs.	Date.	Length of spot.	Obs.
	"			"	
1879, Sept.	10·97	2	1881, Jan.	12·28	1
Oct.	12·58	6	June	11·93	1
1880, Feb.	12·78	1	July	11·51	1
June	11·60	2	Aug.	11·50	2
July	11·61	6	Sept.	11·05	1
Aug.	11·68	2	Oct.	11·34	1
Sept.	11·59	4	Nov.	11·54	2
Oct.	11·55	1	Dec.	10·56	1
Nov.	10·73	1	1882, Jan.	10·65	1
Dec.	11·32	3			

These measures are reduced to mean distance, the equatoreal diameter of Jupiter being adopted as 38"·96, so that the mean length of the spot from all Prof. Hough's individual measures (39 in number for the period now quoted) is 11"·65, which corresponds to 34°·3, and is no less than 5° higher than the mean of Mr. Barnard's 47 estimates, between July 1880 and January 1882. But to be perfectly fair, we must compare only those of Prof. Hough's measures with Mr. Barnard's as were made during the same period, and for these we have a mean of 32°·5, which still exhibits a difference of 3°·2+.

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* I have omitted his obs. on Aug. 3, 1880, and Sept. 29, 1881, as they are obviously misprinted in his work on 'L'Aspect Physique de Jupiter pendant les oppositions de 1879, 1880, and 1881," p. 71 (Bruxelles, 1882).

Summarizing the results of seven observers, we shall be enabled to deduce a very good value for the mean length of the red spot. The observations are not confined to exactly the same periods, but this has very little influence on the results, as the estimates are by no means consistent in regard to the apparent fluctuations in the observed length of the spot :—

Observer.	Length.	Time passing C.M.	No. of obs.	Period of obs.
	°	m		
Barnard......	29·3	48·6	47	July 1880 to Jan. 1882
Denning	33·2	54·9	56	Sep. 1880 to Jan. 1882
Hough	34·3	56·8	39	Sep. 1879 to Jan. 1882
Kortazzi	36·4	60·2	9	Nov. 1879 to Jan. 1882
Niesten......	33·0	54·7	27	Nov. 1879 to Jan. 1882
Pritchett	37·8	62·6	32	July 1880 to Feb. 1881
Williams	32·0	52·9	42	Aug. 1880 to Jan. 1882

If we attach equal weight to each of these results, the mean is 33°·7[*]=56·2 min. from 252 observations. It is evident that Prof. Pritchett's estimates attribute an exaggerated length to the spot, so also Kortazzi's in minor degree, the excess on the mean being 4°·1 and 2°·7 respectively. Mr. Barnard's err the other way, being 4°·4 in defect. Prof. Pritchett allowed the spot 62·6 min. in passing the C.M., while Mr. Barnard gave it 48·6 min., and these are the mean values resulting from a large number of observations. If we compare some of the individual estimates of various observers, we shall find even greater differences, for on July 28 and 29, 1879, M. Niesten gave only 30 min. for the whole transit of the spot, whereas on November 29, 1879, Mr. Gledhill gave 75 min., and on December 6, 70 min.! It is perfectly certain the recorded observations are affected by a good deal of personality, and that many of the supposed variations in length are attributable to errors of estimation. A valuable series of micrometric measures such as those pursued by Prof. Hough afford the best means of eliminating such discordances, though it must be confessed that his individual values are also sometimes not very accordant. Thus, on September 4, 1880, the length of the spot was measured as 10″·90, and two nights later as 11″·95, and it is impossible to conceive that the great difference represents anything but an unavoidable inaccuracy of measurement.

Prof. Hough's mean results for the length during the years 1879–83 are —

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[*] From several observations in Sept. and Dec. 1879, Mr. Backhouse gives 33°·5 as the length of the spot, thus corroborating the mean here given (see 'Monthly Notices,' vol. xl. p. 157). Mr. Brett says—"To the best of my judgment, the major axis of the patch covers 32° of Jovian longitude" ('Observatory,' Vol. iii. p. 238).

1879.	1880.	1881.	1882.	1883.
12″·25	11″·55	11″·30	11″·83	12″·29

The figures appear to indicate that this object is passing through a cycle of variations, but its faintness during the greater part of the last three years must have prevented the continuance of exact observations. The spot has, however, been rapidly increasing in prominence during the last few months, and this may enable us, during the next opposition, to renew observations of the *p.* and *f.* ends with the same facility as in 1879–82. Though definition was bad and the planet's disk small and indistinct, I observed the spot as it passed the C.M. on June 26 and 28, 1885, when it could be seen without difficulty. The light-cloud, recently visible over its centre, appears to have gone and the spot now appears to have recovered much the same aspect as it presented a few years ago, though it is undeniably less conspicuous. I have watched this object, as it passed the meridian of Jupiter, on 205 occasions since 1880, September, and believe that during the whole of that period there has been no real change in its length sufficient to be obvious to the eye.

Bristol, 1885, July 5. W. F. DENNING.

P.S.—With reference to Mr. Lynn's remark in your last number that (as previously pointed out by Arago), "Bayer did uniformly designate the brightest star in each constellation by the letter α," it would seem that, if this plan was scrupulously followed, some remarkable differences in the relative brilliancy of certain stars have occurred since the epoch of Bayer's celestial atlas (1604). β Geminorum is now considerably brighter than α. γ Draconis is more than one magnitude brighter than α and ζ; η and β of that constellation also exceed α. β Herculis is brighter than α, &c. Are these and other instances to be referred to actual variation in the light of the stars, or to looseness in applying the original nomenclature?—W. F. D.

Large v. *Small Telescopes.*

GENTLEMEN,—

Mr. S. W. Burnham has reviewed the question of small *v.* large telescopes in a lengthy article printed in the 'Sidereal Messenger' for September. But it is very unfortunate that he has quite misunderstood the basis of the discussion. The point has been with regard to the relative capacity of large and small instruments in defining markings on bright planets, and not, as he alleges, "their comparative value for showing planetary details and other faint celestial objects."

Mr. Burnham discusses chiefly the merits of certain large refractors on double stars, and while passing a well-deserved eulogium on the instruments of Alvan Clarke and Sons goes into the vexed question as to the relative values of refractors and reflectors. While admitting that personally he "has had no experience in using large reflectors," he says "they are immensely inferior to the refractor in work requiring sharp definition." And, in conclusion, Mr. Burnham has quite misconstrued the very candid statement of Prof. Hall in the 'Observatory' for May 1885, that the large Washington telescope "does not show enough detail."

In point of fact Mr. Burnham has been discussing the whole matter from a standpoint utterly foreign to its original intention. It is therefore unnecessary to follow him unless the controversy is reopened on entirely new lines. What has been said in your journal has not applied to double stars or to comparisons of different forms of telescopes. We have been trying to get at some explanation of the evident failure of large instruments generally in exhibiting detail on bright planets. If Mr. Burnham can furnish anything pertinent to this special question, I shall be obliged to him, but must decline to admit several distinct branches of astronomy into the argument.

The possessors of large telescopes intimate that certain detail visible in much smaller appliances result from inadequate aperture and are, in fact, false appearances. It is replied to this that the superior definition of small instruments amply compensates for what they lack in light-grasping power. This is fully proved by results which admit of no question. It is to small apertures that we are chiefly indebted for our knowledge of planetary markings. The failure of great instruments is not to be explained by the mere assertion that small telescopes introduce pseudo-details which are not recognized with increased power. Observers cannot be too cautious in discriminating between what is real and spectral nor too careful to apply every test with reference to features of new or abnormal character. Want of light, bad seeing, or telescopic misadjustment may occasionally give anomalous aspects to well-known objects or introduce features having no actual existence, so that frequent reobservation is required to eliminate possible errors. But we need not be hypercritical. We are scarcely called upon, in the exercise of undue severity, to obliterate an object which is presented before our eyes night after night with unmistakable distinctness. The explanation has not yet been forthcoming how it is that the 18½-inch Chicago refractor, in the hands of such able men as Profs. Hough and Burnham, gives no trace of the Merope nebula, while Profs. Tempel and Swift, independently, found it so plain in their small telescopes as to mistake it for a comet. Failure in this prominent case may well prepare us for the failures of large telescopes in other respects.

If instruments of about 8 and 10 inches aperture give false impressions of planetary phenomena, they are worse than useless. But they are really condemned for showing too much of what actually exists. In the giant telescopes of the future still less will be seen than in the incomplete views now afforded by the refractors of Chicago and Washington. The belts on Jupiter and the seas on Mars will nearly disappear amid the brilliant, overpowering light from a 10 or 12 feet reflector! There will be blazing disks, affected by incessant moulding and flaring and nearly devoid of reliable markings. Then it will be asserted that the exquisite though relatively small instruments of Alvan Clarke have been guilty of showing too much. Ultimately men will see they do not want light to look at the Sun nor enormous aperture to increase the turmoils of the air. They will come back to small instruments and do better with them, in studying the surface phenomena of bright planets.

Yours very faithfully,
W. F. Denning.

NOTES.

Dun-Echt Observatory Publications.—Lord Crawford has published Vol. iii. of the Publications of his Observatory. It contains Division II. of the account of the Mauritius Expedition to observe the Transit of Venus 1874, Division I. having been issued in 1877 as Vol. ii. of the Publications. The present volume contains the details of the operations for determinations of longitude and latitude undertaken during the Expedition. A very useful *résumé* is given at the end of the book of the positions of several important points, including Berlin, Malta, Alexandria, Suez, Aden, Bombay, Seychelles, Réunion, Mauritius (several stations), and Rodriguez.

Small *v.* Large Telescopes.—We extract the following from an article by Mr. Burnham, in the September issue of the 'Sidereal Messenger,' *in re* Mr. Denning's notes on the relative capacity of large *v.* small telescopes. Referring to the inability of the Chicago 18½-in. refractor to show certain details on the surface of Jupiter, he says :—"The evidence of such changes usually consists in the inability to see a spot or marking on one night which was quite obvious on some immediately preceding or following evening. The argument might have been extended by citing numerous instances of double-stars alleged to have been discovered with small telescopes, and these discoveries verified by other observers, but which have persistently remained invisible in the refractor of the Dearborn Observatory and other large telescopes . . . . in every instance I not only failed to see these alleged stars, but satisfied myself after repeated trials that they were purely imaginary. It never occurred to me that I was labouring under any disadvantage in using a larger aperture than those before employed . . . . That I have as much reason, perhaps, as any one to appreciate the value of small instruments, will be apparent from the work done by the 6-inch refractor . . . Two things are apparent from Mr. Denning's communications: first, that his statements are based wholly on a comparison of small instruments with large reflectors; and, second, that he has never worked with any first-class refracting telescope." In Mr. Denning's letters the following appears:—"While possessing an immense superiority of light over smaller telescopes, they are rendered inaffective by inferior definition." Mr. Burnham is ready to agree with him *in toto* if the remark is applied to reflectors; at the same time admitting his inexperience with the latter class of telescopes he continues:—"The conclusion is irresistible that however well they may answer for work on nebulæ, where there is nothing to define, they are immeasurably inferior to refractors in work requiring sharp definition." Mr. Burnham then proceeds to show the superiority of large refractors in double-star work and concludes that, "Other things being equal, the larger aperture is as much superior to the smaller for observing planetary markings as for difficult double-star work, and for the same reasons." In conclusion, he deals with Professor Hall's letter ('Observatory,' May 1885), the meaning of which he thinks Mr. Denning has failed to see: he says, "I would suggest the propriety of ascertaining directly from him, whether he really took a discouraging, melancholy view of his failure to see so many of the wonders revealed by small telescopes, or whether it was not a delicate form of intimating a wholesale want of faith in the existence of the aforesaid marvels."

CONTRIBUTIONS TO METEOROLOGY. Revised Edition. By Professor LOOMIS.—This is the first portion of a revision of the well-known 'Contributions to Meteorology.' In a preface there is given an interesting account of the circumstances which happily led to the attention of Prof. Loomis being directed to meteorological phenomena and in particular to the subject of storms, and of the opportunities which, after many years, the publication of the weather maps of the Signal Service Department gave him for the more regular investigation of general meteorological laws. In the separate contributions, from time to time published, distinct meteorological questions were treated without, as Prof. Loomis says, regard to systematic order of subjects, in consequence of which later results, derived from a much greater mass of materials, do not entirely harmonize with results published in earlier papers. He has therefore thought it desirable to revise the entire series of papers, and reduce them to a more systematic form, combining therewith new researches on points heretofore neglected, and using not only U. S. observations, but also those from Europe and other parts of the world.

The present first portion of the revised series contains Chap. I., treating of areas of low pressure, their form, magnitude, direction, and velocity of movement; and Prof. Loomis proposes that other portions shall follow as rapidly as his strength will permit. Meteorologists will most heartily wish that he may be enabled to bring his self-imposed work to a successful conclusion.

THE STRASSBURG OBSERVATORY[*].—Dr. W. Schur has published his report of the work done at the Observatory during the last year. The instruments in use at Strassburg are:—(1) the meridian circle; (2) the altazimuth; (3) the great 18-in. refractor; (4) the heliometer; and (5) the 6-in. refractor. It would appear, however, that to work these numerous and excellent instruments Dr. Schur has only two assistants. The meridian circle has been chiefly employed in observing stars for the southern zones of the Astronomische Gesellschaft, as well as in determining positions of the Sun, Moon, and large planets and miscellaneous stars, comet-stars, &c. Determinations have also been made of the errors of division of the circle, and of the form of the pivots. With the altazimuth, to which Herr Schur appears to have devoted a considerable amount of time and attention, observations of the Moon have been commenced. The great refractor has been used for observing Comets, and a re-examination of the micrometer-screw shows the necessity of a correction to the results which have been already published. With the heliometer a number of measures of the Sun's diameter have been made; whilst the 6-inch refractor has been employed in making a series of measures for the determination of the parallax of ψ^5 Aurigæ. The amount of work performed by Dr. Schur and his assistants is very creditable to them; but at the same time we are constrained to remark that it is a misfortune that an Observatory which is so well provided with instruments should not have a larger observing staff, in order that the various instruments might be worked adequately and to the best advantage.

PUBLICATIONS OF THE WASHBURN OBSERVATORY.—Prof. Holden, with the co-operation of Father Hagen, has compiled a Catalogue referred to the epoch 1850·0, from a series of 2161 observations of 1001 stars made by Prof. Tacchini with the Pistor and Martins meridian circle at Palermo in the years 1867, '68, and '69. The stars lie between $-18° 0'$ and $-29° 39'$ of declination. The observations were originally published in the 'Bulletino Meteorologico del R. Osservatorio di Palermo,' which gives the apparent places, together with the epoch. The stars are from the 6th to the 9th magnitude, each having been observed on an average 2·16 times. Prof. Holden states that the observations appeared to be excellent, and he deserves the thanks of astronomers for having rendered them generally available.

Prof. Holden and Father Hagen have also derived the places of 437 southern stars for 1850·0 from Washington Transit-Circle Observations, and with the assistance of Miss Emma Gattiker,

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[*] Astron. Nachrichten, No. 2675.

Miss Alice Lamb, and Mr. J. Tatlock, have compared them with their positions as given in the Cape Catalogue for 1880, the Cordoba Zones, and Yarnall's Catalogue. These comparisons will be of value in deducing the relations of the various systems adopted.

Two independent determinations of the latitude of the Washburn Observatory have been made. The first, by two observers using the zenith telescope according to Talcott's method, gives $43^\circ\ 4'\ 36''\cdot96 \pm 0''\cdot10$ (Holden) and $43^\circ\ 4'\ 36''\cdot98 \pm 0''\cdot09$ (Comstock), or as the result of this method $43^\circ\ 4'\ 36''\cdot97 \pm 0''\cdot07$. The second method, by one observer (Comstock), by transits of stars over the prime vertical, gives $43^\circ\ 4'\ 36''\cdot99 \pm 0''\cdot06$. In this series many of the transits were observed by reflexion, which gives an additional interest to the determination.

These various astronomical papers have been extracted from, and distributed in advance of, 'Publications of the Washburn Observatory,' Vol. iii. They demonstrate very forcibly the energy and ability with which Prof. Holden devotes himself to the service of Astronomy.

THE PARALLAX OF o^2 ERIDANI*.—At the suggestion of M. Struve, Prof. A. Hall has undertaken an investigation of the annual parallax of this remarkable object. The principal star has a proper motion of 4″ a year, at a distance of 82″ there is a double companion which has the same proper motion, while nearly between them is a small star that does not move. Prof. Hall chose as a comparison star a 10·5 mag. star following the principal one $32^{s}\cdot5$ and a little south of it. The observations were made with the filar micrometer of the 26-inch refractor of the Washington Observatory. Between Feb. 23, 1883, and March 4, 1884, 30 observations of these stars were made, each observation consisting of six micrometrical measurements of the difference of right ascension. A discussion of these observations gives, for the relative parallax of o^2 Eridani $\pi = 0''\cdot223 \pm 0\cdot0208$ It will be remembered that Dr. Gill, from heliometer measures, using Lalande 7819 and 8154 of 6·0 and 6·7 mag. respectively as comparison stars (Mem. R. A. S. vol. xlviii. part i. p. 160), found the value $\pi = 0''\cdot166 \pm 0''\cdot018$. The parallax is smaller than one might expect from the nature of this system.

THE OUTER SATELLITES OF URANUS†.—When the 26-inch refractor of the Washington Observatory was mounted in November 1873, the Satellites of Uranus were among the objects first carefully observed with it. The observations were mostly made by Prof. Newcomb during the oppositions of 1874 and 1875; and have been discussed by him in Appendix 1 to the volume of 'Washington Observations' for 1873. The chief result is an excellent determination of the mass of the planet, as well as the elucidation of the facts that the orbits of these satellites are sensibly circular, and that the four satellites move in nearly the same plane. As the Earth would be nearly in the plane of the orbits of the satellites in the year 1882, and since observations at such a time would be valuable for a good determination of the position of this plane, Prof. Hall began a series of observations of the two outer satellites in 1881 which were continued until May 28, 1884. These he has now discussed and published, and in order to make the discussion complete the observations of 1875 and 1876, though distant in date, have also been included. The results obtained by Prof. Hall show that Newcomb's elements require but small correction. If N and J be the node and inclination of the orbit of the satellite on the equator, the final result, assuming that both satellites move in the same plane, is

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* Astron. Nachrichten, No. 2682.

† 'Washington Observations,' 1881, Appendix 1.

$$\left.\begin{aligned} N &= 165^\circ{\cdot}812 + 0{\cdot}0142\,.\,t \\ J &= 75\ {\cdot}300 - 0{\cdot}0014\,.\,t \end{aligned}\right\}\ 1883{\cdot}0,$$

where t is the number of years from 1883·0.

The value of the mass of Uranus, resulting from the deduced corrections to the mean distances of Oberon and Titania, is $\frac{1}{22682 \pm 27}$. The corrections to the other elements may, in Prof. Hall's opinion, be neglected for the present. A favourable time for examining the eccentricities of the orbits will occur about the beginning of the next century; but at that time the planet will be far south, and not favourably situated for observation at northern observatories. On several good nights Prof. Hall searched for new satellites, but none were found.

THE SATELLITE OF NEPTUNE *.—Prof. Hall has discussed the observations of this satellite which have been made by Prof. Holden and himself with the 26-inch refractor of the Washington Observatory, and also those made by Lassell and Marth at Malta in 1863 and 1864; and has deduced corrections to Newcomb's elements of the orbit, published in Appendix 1 to 'Washington Observations' for 1873. The resulting corrections are small. Lassell's period of the satellite, which has been adopted by Newcomb, appears to be slightly erroneous. The question of the eccentricity of the orbit may be more advantageously attacked some ten or twenty years hence, when the apparent ellipse has become more nearly circular, and when there will be a better opportunity to determine this element from the observations. The values of the mass of Neptune, as deduced from the different series of observations, are rather discordant (the mean distances ranging from $16''{\cdot}2699$ to $16''{\cdot}7368$), and appear to indicate the existence of personality in the measures. The value deduced from the mean of Prof. Hall's own observations is $\frac{1}{19092 \pm 64}$.

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* 'Washington Observations,' 1881, Appendix 2.

FAYE'S COSMOGONIC THEORY[*].—We have received the second edition of M. Faye's interesting book 'Sur l'Origine du Monde.' The work is substantially unaltered; some sections have been enlarged, and the latter part of the book has been rearranged, so that it now presents the appearance of a more formal treatise on the subject. The author has introduced some results of spectroscopic observations, Secchi's division of the stars into orders, and other modern developments of astronomical physics, and explains their bearing on the question of the evolution of a Solar system. But M. Faye has seen no reason to retreat from the position he has taken up, so that the remarks published in the 'Observatory,' No. 97, p. 184, are equally applicable to the present edition. He also insists on the accordance of his theory, not only with the Sidereal Astronomy, but also with the Geology of our day. But whether we are prepared to accept M. Faye's conclusions or not, we cannot but admire the vast amount of learning, both historical and scientific, displayed in these pages, as well as the fertility of resource and literary skill with which these stores of learning are utilized as the groundwork of an ingenious and attractive theory.

PROPOSED CHANGE IN THE ASTROMOMICAL DAY.—A correspondent informs us that "the proposal for changing the reckoning of Astronomical time was almost unanimously rejected" by the astronomers assembled at Geneva on the occasion of the recent meeting of the 'Astronomische Gesellschaft.' In our next No. we hope to give a somewhat detailed account of the various discussions which were held during the meeting.

NEW MINOR PLANET.—A minor planet (No. 250) of the 11th mag. was discovered by Dr. J. Palisa on Sept. 3. It was found when searching for (166) Rhodope.

Ephemeris for Tuttle's Comet. By Dr. J. RAHTS[†].

Berlin midnight.	R.A.			Decl.				
	h	m	s	°	′	″	Log Δ.	I.
Oct. 1....	10	13	36·17	−14	27	33·5	0·23178	0·59
5....		27	4·05	17	50	14·0	0·23464	0·56
9....		40	44·54	21	6	42·8	0·23851	0·53
13....	10	54	37·81	24	15	40·8	0·24329	0·49
17....	11	8	43·80	27	16	2·1	0·24891	0·46
21....		23	2·17	30	6	55·0	0·25523	0·42
25....		37	32·28	32	47	42·1	0·26216	0·39
29....	11	52	13·30	−35	18	2·1	0·26956	0·36

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[*] Sur l'Origine du Monde. Théories Cosmogoniques des Anciens et des Modernes, par H. Faye, de l'Institut. Seconde Édition revue et augmentée. Paris: Gauthier-Villars, 1885.

[†] Astron. Nachr. No. 2681.

Elements and Ephemeris of Comet Brooks.

By Dr. E. Lamp.

A new comet was discovered by Prof. Brooks, at Phelps, New York, on Aug. 31. The comet was circular, about as bright as a 9th magnitude star, 2′ in diameter, with some slight central condensation, but no tail.

The following elements have been computed from observations Sept. 5 at Vienna and Paris, Sept. 9 at Hamburg and Kiel, Sept. 13 at Hamburg.

$$T = 1885 \text{ Aug. } 10{\cdot}06230 \text{ Berlin M.T.}$$

$$\left.\begin{array}{lrrr} \pi - \Omega \ldots\ldots & 42^\circ & 17' & 38''{\cdot}0 \\ \Omega \ldots\ldots\ldots & 204 & 40 & 1\ {\cdot}5 \\ i \ldots\ldots\ldots\ldots & 59 & 28 & 29\ {\cdot}2 \end{array}\right\} \text{Mean Eq. } 1885{\cdot}0.$$

Error of middle place (O−C). $d\lambda \cos\beta = -10''{\cdot}4$. $d\beta = +3''{\cdot}1$.

Ephemeris for Berlin Midnight.

	App. R.A.			App. Dec.		Log Δ.	Log *r*.	Brightness.
	h	m	s	°	′			
Sept. 29..	16	45	43	+42	50·1	0·0089	0·0832	0·64
Oct. 1..	16	59	0	+42	32·7			
3..	17	14	9	+42	8·5	0·0118	0·1025	0·58
5..	17	29	8	+41	37·7			
7..	17	43	48	+41	0·9	0·0178	0·1211	0·52
9..	17	58	7	+40	18·5			
11..	18	12	2	+39	31·2	0·0266	0·1390	0·46
13..	18	25	31	+38	39·8			
15..	18	38	30	+37	44·7	0·0384	0·1563	0·40
17..	18	50	59	+36	47·0			
19..	19	2	56	+35	47·1	0·0528	0·1730	0·35
21..	19	14	23	+34	45·6			

The brightness on Sept. 5 is taken as unity.

Differences of Right Ascension and Declination between Iapetus and the Centre of Saturn. By A. Marth *.

Iapetus − Saturn.

Greenwich Noon.	α−A.	δ−D.
	s	″
Nov. 1	−38·6	− 59
2	39·4	70
3	40·0	81
4	40·2	92
5	40·3	102
6	40·0	111
7	39·4	120
8	38·6	128
9	37·5	135
10	36·1	141
11	34·5	146
12	32·6	150
13	30·6	154
14	28·3	156
15	−25·8	−157
Nov. 16	−23·1	−157
17	20·3	156
18	17·3	154
19	14·2	151
20	11·0	147
21	7·7	143
22	4·4	137
23	− 1·0	130
24	+ 2·4	122
25	5·8	114
26	9·2	105
27	12·5	95
28	15·7	84
29	18·9	73
30	+21·9	− 61

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* Monthly Notices, Vol. xlv. No. 8.

Maxima and Minima of Variable Stars in November 1885.

M, signifies maximum ; *m*, minimum.

Nov.	1	U Monocerotis, M.
	1	W Herculis, M.
	1	T Hydræ, M.
	1·3	T Monocerotis, M.
	2·5	ζ Geminorum, M.
	5	S Cygni, M.
	6	T Ophiuchi, M.
	7·6	ζ Geminorum, *m*.
	10	R Lyræ, *m*.
	10	R Arietis, *m*.
	10	T Cancri, *m*.
	11	R Sagitta, *m*.
	11	S Coronæ, *m*.
	12·7	ζ Geminorum, M.
	15	R Virginis, M.
	15·7	β Lyræ, *m*.
	17	R Camelopardi, M.
Nov.	20	R Canis Minoris, M.
	20·4	T Monocerotis, *m*.
	21	S Virginis, M.
	21	R Persei, M,
	23	U Monocerotis, *m*.
	23	T Ursæ Majoris, *m*.
	23	U Cancri, M.
	24	R Crateris, M.
	24	S Vulpeculæ, M.
	25	R Lyræ, M.
	26	R Coronæ, *m*.
	27	R Delphini, M.
	27	R Boötis, M.
	28·3	T Monocerotis, M.
	28·6	β Lyræ, *m*.
	29	R Ceti, M.
	30·4	W Virginis, M.

Variables of Short Period.

δ Cephei.

	d.
Nov.	2·75, *m*.
	4·35, M.
	9·75, M.
	13·45, *m*.
	20·45, M.
	24·25, *m*.
	29·55, *m*.

U Sagittarii.

	d.
Nov.	1·3, *m*.
	14·7, *m*.
	17·7, M.
	21·5, *m*.
	24·5, M.

W Sagittarii.

	d.
Nov.	1·7, *m*.
	9·3, *m*.
	12·5, M.
	24·5, *m*.
	27·7, M.

X Sagittarii.

	d.
Nov.	3·3, *m*.
	10·3, *m*.
	17·3, *m*.
	24·3, *m*.

U Coronæ, *m*.

		h	m
Nov.	4	12	19
	11	10	2
	18	7	44
	28	16	18

η Aquilæ.

	d.
Nov.	5·7, *m*.
	15·3, M.
	22·4, M.
	29·6, M.

U Cephei, *m*.

		h	m
Nov.	3	17	10
	8	16	49
	13	16	29
	18	16	8
	23	15	48
	28	15	27

S Cancri, *m*.

		h	m
Nov.	13	5	59
	22	17	37

Algol, *m*.

		h	m
Nov.	8	15	37
	11	12	26
	14	9	15
	28	17	20

λ Tauri, *m*.

		h	m
Nov.	3	16	3
	7	14	55
	11	13	48
	15	12	40
	19	11	32
	23	10	25
	27	9	17

Satellite of Neptune.

Greenwich Mean Times of the North-east Elongations.

Nov. 2....	$21^{\mathrm{h}}{\cdot}3$	Nov. 14....	$15^{\mathrm{h}}{\cdot}5$	Nov. 26....	$9^{\mathrm{h}}{\cdot}7$
8....	18·4	20....	12·6		

Position-angle of minor axis of apparent orbit on Nov. 1 = 321° 28′; on Dec. 1 = 320° 50′.

Semi-major axis of orbit on Nov. 1 = 16″·96; on Dec. 1 = 16″·95.

A diagram showing the apparent orbit is given on p. 318.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can usually be only seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS (East).

		h
Nov.	1	11·0
	2	9·6
	12	18·4
	13	17·0
	14	15·6
	15	14·2
	16	12·8
	17	11·5
	18	10·1
	19	8·7
	29	17·6
	30	16·2

MIMAS (West).

Nov.	4	18·3
	5	16·9
	6	15·5
	7	14·1
	8	12·7
	9	11·3
	10	9·9
	11	8·5
	25	11·9
	26	10·5
	27	9·1
	28	7·7

ENCELADUS.

		h
Nov.	1	12·0
	2	20·9
	4	5·8
	5	14·6
	6	23·5
	8	8·4
	9	17·3
	11	2·2
	12	11·0
	13	19·9
	15	4·8
	16	13·7
	17	22·6
	19	7·4
	20	16·3
	22	1·2
	23	10·1
	24	19·0
	25	3·8
	27	12·7
	28	21·6
	30	6·5

TETHYS.

		h
Nov.	2	11·7
	4	9·0
	6	6·3
	8	3·6
	10	0·9
	11	22·1
	13	19·4
	15	16·7
	17	14·1
	19	11·4
	21	8·7
	23	6·0
	25	3·3
	27	0·6
	28	21·9
	30	19·1

TITAN.

Nov.	3	21·0
	19	18·7

HYPERION.

Nov.	5	19·1
	26	23·6

DIONE.

		h
Nov.	1	6·6
	4	0·2
	6	17·9
	9	11·6
	12	5·3
	14	22·9
	17	16·6
	20	10·2
	23	3·9
	25	21·5
	28	15·2

RHEA.

Nov.	2	15·1
	7	3·5
	11	15·7
	16	4·1
	20	16·4
	25	4·8
	29	17·1

Apparent Elements of the Ring.

Greenwich Noon.	Position-angle of Minor Axis.	Latitude above plane of Ring.	
		Earth.	Sun.
1885, Nov. 4	.... 353° 8′	25° 24′ S.	26° 18′ S.
19	.... 353 10	25 31 S.	26 14 S.

Greenwich Noon.	Outer Ring.		Inner Ring.		Semi-maj. axis of Orb. of Titan.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	
1885, Nov. 4	.. 44″·48	19″·09	29″·58	12″·70	200″·1
19	.. 45 ·47	19 ·59	30 ·24	13 ·03	204 ·8

The semi-major axis of the orbit of Titan is given as well as the position-angle of the minor axis of the ring, in order to furnish means for roughly inferring the distances and position-angles of the satellites from the diagram given on p. 316.

Astronomical Memoranda, November 1885.

Equation of Time:—Sun *before* Clock, Nov. 1, 16^{m} 19^{s}; Nov. 11, 15^{m} 48^{s}; Nov. 21, 13^{m} 53^{s}; Nov. 30, 11^{m} 2^{s}.

Sidereal Time at Mean Noon:—Nov. 1, 14^{h} 43^{m} 45^{s}; Nov. 11, 15^{h} 23^{m} 10^{s}; Nov. 21, 16^{h} 2^{m} 36^{s}; Nov. 30, 16^{h} 38^{m} 5^{s}.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
Nov. 2....	18 59	4 31	24 22	+4 1	270 18
7....	19 7	4 23	23 21	3 29	204 24
12....	19 16	4 16	22 8	2 54	138 28
17....	19 25	4 9	20 44	2 19	72 33
22....	19 33	4 3	19 9	1 42	6 39
27....	19 40	3 57	17 23	+1 4	300 45

The Moon.

	rises. h m		*sets.* h m		*sets.* h m		*rises.* h m
Nov. 1	13 25	Nov. 9	6 27	Nov. 17	14 16	Nov. 25	7 34
2	14 39	10	7 14	18	15 25	26	8 46
3	15 52	11	8 6	19	16 36	27	10 1
4	17 5	12	9 1	20	17 50	28	11 15
5	18 15	13	10 0	21	19 4	29	12 29
6	*sets*	14	11 1	22	*rises*	30	13 42
7	5 9	15	12 4	23	5 31		
8	5 45	16	13 9	24	6 28		

Nov. 6^{d} 9^{h} 3^{m} New Moon. Nov. 14^{d} 10^{h} 0^{m} First Quarter.
21 21 39 Full Moon. 28 13 57 Last Quarter.

Occultations for November 1885.

Day.	Star.	Mag.	Disap.	Angle.	Reap.	Angle.
			h m	°	h m	°
17	B.A.C. 8365	6½	10 41	140	11 44	266
22	θ^1 Tauri	4½	6 29	73	7 17	313
22	θ^2 Tauri	4½	6 38	46	7 9	340
22	75 Tauri........	6	6 49	173	7 8	213
22	B.A.C. 1391	5	7 18	101	8 17	285
22	Aldebaran	1	9 48	107	10 57	278
23	117 Tauri	6	5 59	66	6 42	310
23	130 Tauri	6	14 52	70	15 58	290
24	26 Geminorum ..	5½	10 59	92	12 6	271
25	1 Cancri	6	17 40	78	18 46	255
30	URANUS	..	17 0	81	18 9	231

Planets for November 1885.

Day.	Planet.	R.A.	Dec.	Diam.	Rises.	Tr.	Sets.
		h m s	° ′	″	h m	h m	h m
1	Mercury..	15 5 35	18 24 S.	4·8	20 2	0 22	4 49
11		16 7 33	22 51 S.	5·0	20 52	0 44	4 42
21		17 9 22	25 23 S.	5·6	21 30	1 7	4 47
30		17 58 47	25 44 S.	6·4	21 42	1 21	4 56
1	Venus ..	17 29 40	25 49 S.	17·6	23 12	2 46	6 22
11		18 20 28	26 10 S.	19·0	23 26	2 57	6 31
21		19 9 56	25 22 S.	20·8	23 28	3 7	6 47
30		19 52 13	23 44 S.	22·6	23 22	3 14	7 6
1	Mars	9 56 28	14 18 N.	6·9	11 53	19 11	2 29
11		10 16 43	12 39 N.	7·3	11 43	18 52	2 3
21		10 35 39	11 2 N.	7·8	11 31	18 31	1 34
30		10 51 26	9 38 N.	8·2	11 19	18 12	1 7
1	Jupiter ..	11 52 20	2 2 N.	30·3	14 52	21 6	3 23
11		11 58 58	1 20 N.	30·7	14 22	20 33	2 48
21		12 5 3	0 43 N.	31·3	13 53	20 0	2 11
30		12 9 59	0 13 N.	32·0	13 24	19 29	1 37
1	Saturn ..	6 35 45	22 18 N.	17·6	7 40	15 49	23 58
30		6 29 58	22 23 N.	18·3	5 40	13 49	21 59
1	Uranus ..	12 21 23	1 34 S.	3·6	15 38	21 34	3 34
30		12 26 25	2 5 S.		13 51	19 45	1 43
1	Neptune..	3 30 44	17 12 N.		5 10	12 45	20 20
30		3 27 27	17 0 N.		3 13	10 47	18 21

Nov. 15^{d} 20^{h}. Neptune in opposition.
30 12 Mercury at greatest elongation (21° 14′ E.).

Phenomena of Jupiter's Satellites, November 1885.

d	h	m		d	h	m		d	h	m	
4	17	1	i. Ec. D.	14	17	0	iv. Oc. R.	26	16	2	iii. Tr. E.
5	17	18	i. Tr. E.	15	17	51	iii. Ec. R.	27	17	9	i. Ec. D.
7	16	54	ii. Ec. D.	16	16	35	ii. Tr. I.	28	15	23	i. Tr. I.
8	17	40	iii. Oc. R.	20	15	16	i. Ec. D.		17	39	i. Tr. E.
9	16	42	ii. Tr. E.	20	18	34	i. Oc. R.	29	15	0	i. Oc. R.
12	16	59	i. Tr. I.	21	15	42	i. Tr. E.				
13	16	37	i. Oc. R.	25	16	19	ii. Oc. R.				

The R.A. and N.P.D. of Planets and the Solar Co-ordinates are given for Greenwich Mean Noon; in all cases Greenwich Mean Time is used, reckoning from noon to noon, as in previous years; the angles for Occultations are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 103. NOVEMBER. 1885.

Meeting of the Astronomische Gesellschaft.

THE twelfth General Meeting of the International Astronomical Society (Astronomische Gesellschaft) was held at Geneva in August last, and was attended by about 40 astronomers from Europe and America. A brief account of the proceedings at this meeting may be given here.

The society met for the first time on Wednesday, August 19, and this day's session was absorbed for the greater part by official reports on the scientific undertakings which are progressing under the auspices of the society. Amongst these the first in rank is the observation in zones of the stars of the northern heavens, between the parallels of $\delta+80°$ and $-2°$, down to the 9th magnitude. It was stated that at each of the thirteen cooperating observatories the observations were finished or nearly so, and that also the reductions were going rapidly forward. Prof. Krüger of Kiel, formerly at Helsingfors and Gotha, has already published the whole of his observations (zone 65°–55°): of those made at Leiden (zone 35°–30°) the first part is published and the other will very soon follow, and besides these the zone observed at Kasan (80°–75°) is also in the course of being printed.

When the northern zone observations have been completed, it is proposed to continue them below the equator and to go as far as 23° of southern declination. The preparations for this extension form the second great work with which the "Gesellschaft" is interested. They consist of two distinct parts:—1. The so-called southern "Durchmusterung," a continuation, after precisely the same plan, of the well-known great work of Argelander, which has been taken in hand by his successor Prof. Schönfeld; 2. The observation of a catalogue of 303 fundamental stars, which will form the points of reference for the zone-stars. As to the first point, the meeting received from Prof. Schönfeld the highly important announcement that not only was his work completely finished, but that it was already in print and would appear before long. The new fourth section of the 'Bonner Sternverzeichniss' from $-2°$ to

$-23°$ contains 133,658 stars, which will also be mapped down on charts in the same manner as those of the preceding ones.

The observations of the fundamental stars are to be made at six observatories situated in both hemispheres—Cape of Good Hope, Leiden, Madison (Wisconsin, U.S.), Strassburg, Melbourne, and Naples, and reports were read on the progress of the work. Mr. Gill has already succeeded in completing his share in it (a minimum of twelve observations for each star) and only some special investigations remain to be made. At Leiden, Madison, and Strassburg the work is steadily advancing; and as for Melbourne and Naples, it was reported the observations would begin very soon. At Melbourne this beginning has been delayed until the new transit-circle shall have been mounted. In addition to the results directly in view it is hoped that this cooperation of northern and southern observatories will also materially advance our knowledge of refractions; and for this purpose to the catalogue of fundamental stars proper there have been added some other stars, so as to form an uninterrupted series extending from the zenith of the northern to that of the southern observatories.

Reports were also read on two other subjects: the first by Herr Wolff, of Bonn, on the photometrical work that is to be done by him in accordance with the views expressed by the committee on standards of stellar magnitude, which was appointed by the American Association, the Royal Astronomical Society, and the Astronomische Gesellschaft; and the second by Prof. Weiss, of Vienna, on the present state of cometary calculations. Whilst explaining the general outlines of his intended work (Zöllner's photometer will be used), Herr Wolff also directed attention to several undecided questions, as, for instance, whether it would be better to distribute a given number of observations on a greater or a smaller number of stars. In the report of Prof. Weiss the results were embodied of a very thorough examination of all the calculated orbits of comets, and statistics were given of those which could be regarded as definitive and those which would require further investigations. Prof. Weiss reported also on the periodic comets; and with regard to their rapidly increasing number he expressed the opinion that it would in future become necessary to have a special bureau to take care of them. On this point Prof. O. Struve expressed his disagreement with Herr Weiss: to the routine work of a bureau he would always prefer the initiative of individual astronomers.

The last occurrence at the first session was the reading of a note by Herr Schram, of Vienna, on the great work of Prof. Oppolzer ('Canon der Sonnenfinsternisse') which will presently appear, containing approximate calculations for all the solar eclipses between the years B.C. 1200 and A.D. 1900. Certain additions to this work had been made by himself.

The first part of the second session, on Thursday, August 20, was occupied with internal business of the society. As the only point of more general interest we may mention here, although the voting was delayed till Saturday, that it was decided the next meeting should take place in 1887 at Kiel.

Then the President, Prof. Auwers, opened a discussion on the 6th resolution of the meridian conference at Washington, in so far as it expresses the hope that the astronomical day will be arranged so as to begin at mean midnight. Some months before the council had considered the question and with a great majority (only Prof. Oppolzer contra) they had pronounced themselves against the desirability of the change. And now, although the society as such was not entitled to give a vote on the subject, the President thought it desirable that as many individual astronomers as possible should expound their views.

The first to respond to this appeal (Prof. Oppolzer was not at Geneva) was Prof. O. Struve, who declared himself a partisan of the proposed change. As, now-a-days, astronomers make almost as many observations in the daytime as during the night, the inconvenience of changing the date during a series of observations would not be much greater with the new than with the old mode of reckoning. He did not deny that the discontinuity in astronomical ephemerides and in astronomical reckoning would cause some inconvenience, but he thought this was a sacrifice astronomers ought to make for the benefit of science in general.

Several other members, amongst whom were Profs. Newcomb, Auwers, Krüger, Weiss, Tietjen, expressed a different opinion. They thought that, were astronomy an entirely new science without any documents transmitted to us from the past, it might be dubious which of the two systems would have the greatest advantages; but now, as things are, the troubles and confusion arising from an alteration occurring in the otherwise continuous series of our observations and ephemerides, supposing even that the change would be made at the same moment throughout, would certainly not be counterbalanced by its advantages. And as for the cases of other scientists coming to use astronomical data, it would, as they are comparatively few in number, not be very troublesome to astronomers then to make the necessary reductions.

Prof. Gyldén stated that his views on the subject differed somewhat from the last quoted, inasmuch as he would see no objection to making the change when astronomers of the whole world should have agreed on the point. Dr. Pechüle, of Copenhagen, was of the same opinion. As was said before, no vote ensued, but the great majority of the expressed opinions had been against the proposed change.

In the afternoon of Thursday different papers on scientific subjects were brought before the society. Prof. Mittag-Leffler, of Stockholm, explained the subject of a prize-essay, proposed by a committee appointed by the King of Sweden and consisting of a very general problem of celestial mechanics. Mr. Struve gave an interesting account of his first experiments with the new 30-inch refractor at Pulkowa, and declared himself highly satisfied with the optical and mechanical qualities of the instrument. Prof. Newcomb, who had been for some time in Pulkowa, fully confirmed his statements.

M. Tisserand, of Paris, explained a new mode of treating the problem of the Earth's rotation. Following the lead given by Prof. Gyldén in his theory of celestial motions, he had retained in the first approximation some of the more important terms arising from the perturbative forces, and had so obtained a more general solution. Dr. Steinheil, of Munich, spoke on an improved construction of the Galilean telescope. He had succeeded in diminishing considerably the spherical and chromatic aberrations and showed some specimens of what he had been able to do. The last speaker was Prof. Spörer, of Potsdam, who discoursed on the elements of rotation of the Sun and the origin of solar spots, and advocated a theory of ascending and descending currents towards and from the Sun's surface.

At the third and last session, on Saturday, August 22, after some votes on business of the society and new elections for part of the Council (all the members were re-elected and the Council will, as before, consist of Profs. Auwers, Gyldén, H. G. van de Sande Bakhuyzen, v. Oppolzer, Weiss, Schönfeld, Seeliger, and Bruns), the scientific proceedings continued.

Prof. Gyldén spoke on some researches in the domain of the asteroids made by Dr. Harzer. He has treated after Gyldén's new methods the theory of Hecuba, a particularly interesting planetoid, inasmuch as its mean motion is nearly commensurable with that of Jupiter, a circumstance that must necessarily vitiate the results of the ordinary methods of the variation of constants. Dr. Harzer has also made some statistics on the asteroids in general, arranging them in order of their mean motions. The remarkable circumstance first pointed out by Mr. Kirkwood and afterwards investigated by Prof. Hornstein, that the series presents discontinuities at the regions of commensurability of mean motion with that of Jupiter, was shown anew with great clearness.

Prof. Newcomb gave an interesting account of the present state of the great work he has undertaken, viz. the reinvestigation of the planetary motions on an uniform plan. He is now chiefly occupied with the theories of the Sun, Mercury, Venus, and Mars, which are to be carried on simultaneously. Much has already been done, as well in the way of calculating perturbations as in that of re-reducing the older observations and comparing them with Leverrier's tables, but still a vast amount of work remains. A discussion with Prof. Gyldén on the methods of calculating perturbations ensued. Prof. van de Sande Bakhuyzen, of Leiden, spoke on his recent researches on the planet Mars, based on the drawings of his surface made from the time of Huygens and Hooke until now, and particularly on those of Schröter, which were published recently by him. He finds for the period of rotation $24^{h}\ 37^{m}\ 22^{s}{\cdot}66 \pm 0^{s}{\cdot}013$, but at the same time has rendered it probable that during the last century some changes have occurred on the planet's surface.

Dr. Müller, of Potsdam, read a paper on photometrical methods generally and on his own recent work in this branch of science, which gave rise to a discussion on these methods, in which several of the members took part, and which proved anew that, notwithstanding the remarkable publications of recent years, celestial photometry is a science still in its infancy. Of Dr. Müller's own results, we may record here interesting ones of observations on some of the brighter asteroids. Pallas, for instance, showed clearly the influence of defect of illumination, which, in consequence of the great inclination of its orbit, may be considerable.

Prof. Seeliger, of Munich, read a note on Lambert's law of the emission of light and the reflection from illuminated surfaces; he concluded that this law has no theoretical foundation and practically does not hold good in any case. The last speaker was Prof. Weiss, who presented to the society a catalogue of stars which he had constructed from observations made at Geneva by the former director Mr. Plantamour, between the years 1841 and 1858.

After thanks had been voted to all who had contributed towards rendering the sojourn at Geneva a very agreeable one, and in particular to M. Gautier, director of the observatory, the session was closed.

The Spectroscope in the Alps.

As holiday occupation this year in Switzerland, I decided on trying to make some contribution toward the solution of the vexed question of the origin of the A and B systems of the solar spectrum. The place selected was Davos, as affording excellent quarters at an elevation of 5200 feet above sea-level, and with dry air and clear sky. The instruments taken out were:—an automatic solar spectroscope, by Browning, of ten prisms dispersion; a heliostat and all necessary meteorological appliances; and a 6-in. Calver equatoreal. During the period of observation, July 10 to August 24, the weather was very favourable; and the peculiar haziness which last year baffled the corona photographers upon the Riffel was on many of the days almost or entirely absent. Observations of the solar spectrum were taken for the most part at about noon, at a maximum solar altitude of from 55° to 65°, and, for contrast, not far from sunrise or sunset. By way of special training, the lower end of the spectrum had been made the subject of almost daily study for many weeks previously, at an elevation of about 150 feet above the sea.

On setting to work, with a high sun, under a beautifully blue sky, it was at once evident that the red had become much more difficult to get at, while the violet was rendered far easier; and these conditions proved throughout to be directly proportional to the blueness of the sky. Likewise, the B system gave the impression of being, for the solar altitude, less developed but more sharply defined. Its lines appeared thinner, and indeed fewer. The attached band was paler, and the lines were not easy to make out. The A system was not accessible enough for any trustworthy observation. Such first impressions of these formations, as seen in a high sun, were confirmed by all subsequent experience. In a low sun, say of 8° altitude, the difference was great indeed, especially in the clear air of early morning. The whole wonderful sequence of doublets in the B preliminary would then stand out with a precision I had never seen equalled or approached, as also the lines in the attached band; and in both cases with a curious insistance, so to say, on showing the clear red background between even the closest pairs. The well-known gap in the B system was at such times filled in with very delicate striation, and occasionally the entire space from A right up to the beginning of B preliminary would be seen crowded with distinct lines, free from all haze, the "little *a*" system, grandly developed, as clean and pure as the rest. The A preliminary would then also show plainly enough; but the breaking up of these doublets was too much for my instrument. Such presentments of A and B appeared to be wholly independent of variation in the amount of water vapour in the air, which ranged from almost complete saturation to (July 29) only 34 per cent. Yet it must be noted that among the mountains the values obtained by the hygrometer are apt to be most illusory, unless for a very limited distance from the place of observation. This is especially the case at Davos, where lateral valleys, each headed by a glacier, debouch upon the main depression.

In due time I proceeded to try the effect of increasing the height of the observing station by yet another 5000 feet; and, from the Fluela Hospice, made the ascent of the Schwarzhorn, 10,335 feet above the sea. Nothing could be imagined more perfect than the atmospheric conditions of that day, July 24, the view extending from the Bernese Oberland to the Gross Glockner on the far side of the Austrian Tyrol, with every peak visible all round the horizon. A Swiss officer of Engineers ascended the same day with his men, to get some distant theodolite angles, and rendered most friendly assistance. He quite agreed with me that, on that day, no trace whatever was to be seen of the notorious "meteoric" haze of the last two years. The Sun stood sharp and clear upon a sheet of the most superb azure, pure right up to the disk. Four hours, from 10 A.M. to 2 P.M., were spent at the spectroscope, which I had had fitted, before leaving England, with an arm carrying a 3-in. condensing lens in front of the slit, and with rack-screws for following the apparent motion, for better observation of the Sun direct, the management of the light being all-important when near the limits of vision. Also I added a swing-shutter with a red glass, far enough from the slit to escape being broken by the heat. But for this, which is due in principle to a suggestion of Professor Piazzi Smyth, the chief work would have been most seriously hampered by the outstanding violet rays. I have had a red cap fitted thus to a pocket instrument, and can safely say that no one who has tried such an appliance upon a pocket-spectroscope will ever again consent to be without it, giving as it does a spectrum right down to great A perfectly defined. For the other end of the spectrum no suitable violet glass appears to be known in the trade; but a blue shade will often decidedly improve the seeing.

The general view of the spectrum was very striking in several respects. In the first place, the colours were singularly vivid, with something of that peculiar brilliancy which will occasionally characterize an Alpine rainbow. Then, again, the sharpness of the definition was notable, as though it had become impossible for the observer to hesitate whether a line was visible or not. For instance, the great iron line below F, heretofore just separable and no more, now came out as a perfectly hard doublet with a streak of blue-green dividing the lines from top to bottom. Another feature, brought out to a surprising extent, was the contrast between the furred lines (if I may so call them) and the sharp ones. Here, as in the last case, there was in the spectrum a *willingness*, so to say, which made the examination of it a real luxury. The great magnesium group was a noble sight: with b_1 broad and dense, much more furred than b_2, and this latter rather more so than b_4, all contrasting with the perfect clearness of b_3 and of the other lines adjacent. The only iron line which showed any furring was E, where it was tolerably plain. Hydrogen F appeared more thickly furred than C, while "near G" was just perceptibly. The D lines were of course hazy enough, though not to the extent of the magnesium.

In respect of these D lines, it may here be noted that no difference between them in intensity could now be detected, though D_1 was, as usual, the narrower of the two. I can nowhere find mention of the remarkable change which these two sodium lines undergo in a very low sun; though, doubtless, matter of common observation. All semblance of companionship is lost; D_2 decidedly gaining in force, after the manner of telluric lines, and D_1 as decidedly failing. The strengthening might perhaps be ascribed to atmospheric sodium increasing the absorption; but the enfeeblement is harder to account for, as also the selective process, in that the lines are conversely affected. At first it was natural to suppose that the apparent failure of the lower line was an effect of contrast, just as the sun-spot showed brown when Venus in transit came upon the disk. I therefore fitted a broken needle in the focus of a positive eyepiece, to serve as an occultation-bar; and the result of observing with it has been to force upon me the belief that D_1 really does darken inversely as the depth of the atmosphere. I have examined the other sodium pairs, at various solar altitudes, without finding in them any sign of these variations. The three pairs available for such purpose are well placed as regards light, and are easy to identify; but in all cases these lines would appear to be so complicated by coincidences with the lines of other metals, that nothing short of a very clear agreement of testimony amongst them could be held sufficient to establish any certain conclusion.

The chief interest, however, centred on B. The whole system was now weakened down precisely as in the case of the α group in a moderately high sun. The attached band was pale, very pale, with barely a trace of striation. The upper edge, or B proper, appeared as a bar not nearly so dark as C, and with that irresolute aspect which an object puts on when upon the point of breaking up. The lines of the preliminary series were perfectly well defined, but very thin, and some of the lower ones seemed to have faded out. The light would not bear a high enough power for counting them with any degree of confidence. Great A was bold and strong, but that is all I can say, the deep red defying all penetration with a narrow slit, in the intensely blue noonday sky. Not a line could be seen all the way from the upper edge of A to the beginning of the B preliminary, "little a" being merely indicated by a slight darkening. The slit was, however, for this observation, a wide one, though not too wide to separate the B series.

The α group was practically gone. By setting the pointer for its known position I saw a line in the place of its upper edge; but whether α or Kirchhoff's gold I cannot tell.

In the D space, nothing was to be seen of the variable lines, except a very faint trace of the strong one between the nickel and D_2; the instrument showing five of these lines, besides the nickel, in low sun and moderately damp air. The δ group above D was always plainly visible, appearing but little affected by the increased terrestrial altitude. Of its three component fasciculi, the lowest was weaker than the others, the which I note because it is this edge of the group that in a low sun is the darker. Higher than δ I could see no sign of other of the Brewster-Gladstone series.

The violet end of the spectrum was beautifully developed. Every detail of the H space was displayed as though one were merely dealing with the lines about G; and above this the eye travelled on into regions which, in a refraction spectrum, seemed marvellously remote. The illusion was sadly dispelled when the measures came to be plotted out upon a curve of wave-lengths, the ultimatum of distinct vision proving to be merely an iron line at λ 3870 of Cornu's map. The limiting cause was not want of light, but exhaustion of the refractive powers of flint glass. A quartz train under that sky would have shown wonders indeed.

As a weight test, instrumental and observational, I may mention that three independent measures, taken from different datum lines in the H space, without the smallest idea how far they might agree, and not reduced until a few days later, gave respectively 357·5, 358·0, and 359·0 micrometer units as the distance of the ultimate line above H_1. It will be rightly inferred that I was observing at ease, local circumstances lending themselves singularly well to that desirable end. Indeed, for long sustained work at an eyepiece, bodily comfort is of such importance to accuracy of observation, that reasonable provision for it in the way of rugs &c. should never be neglected.

At noon, solar radiation *in vacuo* was 67°·3 as against 58°·5 at the same time at Davos; shade temperature was 42°·3 as against 71°·5—all Fahrenheit. The percentage of humidity was 52 as against 34 at the lower level. The hygrometer readings were doubtless affected by evaporation from the glacier below us. At 11.25 A.M. there was as much as 74 per cent. of water vapour, though the disturbance was of short duration, and probably of very limited range. With such surroundings, the evidence of the water lines in the D space is more to be relied upon than that of any hygrometer, provided it can be got at; and from what has been already said concerning those lines, it would seem that the air on this day was most particularly dry. The thermometers, except the solar radiation, which was lashed to an alpenstock, were behind a Glaisher screen, for the construction whereof suitable material had been taken up. They gave the maximum of radiation precisely at noon, though all the readings rose a few degrees subsequently. Barometer, corrected for temperature and reduced to sea-level, was 30·374 inches.

In addition to the observations on the Schwarzhorn, the groups in the red were examined on other days at several intermediate heights—as the Kupfenfluh, 8668 ft.; the Fluela Pass, 7838 ft.; the Strela Pass, 7800 ft., twice; the Schatzalp, 6400 ft., three times; Sertig Dörfli, 6103 ft., &c., &c. Also, for lower levels, at Chur, 1919 ft., and at Landquart, 1739; as likewise, afterwards, in the neighbourhood of Brunnen, on the Lake of Lucerne, and then upon the Rigi. The testimony was perfectly harmonious throughout, to the purport that the B group is of similar nature to the α group; that is to say, that it is not solar at all, nor in any way of cosmical origin, but is a true atmospheric sequence, developed in direct proportion to the density of the air, and independently of the amount of water vapour in suspension.

Moreover, the analogies between the A, B, and α groups are such as to suggest a close relationship. In every case we have a system with two principal members, separated by a bold gap. The upper member is a fasciculus of lines for the most part, if not entirely, in doublets more and more densely packed as the wavelength decreases, and ending abruptly in a well-defined edge, invariable in position, always the most conspicuous feature of the whole group, and the last to fade or disappear. The lower member is a sequence of much wider doublets, indefinite below, and, in A and B, very remarkably terminated above by one single line. All the three groups gain together and lose together in intensity and general development, according to the length of air-path traversed by the light. These things considered, there is not a little to be said for the surmise that in A, B, and α we have three bands of a so-called channelled spectrum, seen superposed upon the solar spectrum proper. And, further, just as among the more sharply defined bands in such a spectrum we occasionally see other bands bearing to them a merely general resemblance, so in this case we have in the "little *a*" group the rudiments of a system thus related, with its dark upper member and lighter lower one and gap intervening. And possibly, too, there is in the behaviour of the D system enough to plead some excuse for an observer who should find his thoughts wandering in this direction while studying its developments already mentioned. As the upper line gathers greatness, and an attached band accrues, the eye glances across the clearer space below the nickel at the doublets crowding up on the other side of the gap, and the imagination is apt to become unruly. Yet the student of the solar spectrum should, of all men, least need to be reminded that "Dry light is best."

What we now seem to want is a series of observations of the lower end of the spectrum at sunrise and sunset, at maximum atmospheric pressure, and preferably in winter, in extreme cold, in an air as dry as may be, to get rid of the aqueous lines. It is far from impossible that a fourth member of this series may thus be detected, bearing the same relation to *a* that *a* bears to B in its turn; nor may any one venture to say that the day will never come when the places of such bands may be computed at the desk with as much certainty as the spacing of the frets upon a violin.

In the pleasant holiday work above detailed, I have found the utmost help from the truly classical studies of certain portions of the solar spectrum set forth in the 'Madeira Spectroscopic' of Prof. Piazzi Smyth, a copy of which I owe to the courtesy of that distinguished observer. Likewise much assistance has been derived from the maps and tables in the new and excellent 'Lehrbuch der Spektralanalyse' of Prof. Kayser, now of Hanover. It is but right also that I should gratefully acknowledge the enlightened cooperation of the proprietor of the Hotel Belvedere at Davos, Herr J. C. Coester, who himself possesses a series of thermometers of the highest excellence, together with a singularly fine standard barometer, all Kew tested, and who keeps their daily record with an accuracy which leaves nothing to be desired.

Clevedon, October 1885. STEPHEN H. SAXBY.

Astronomical Notes.

COMET BROOKS (1885 *b*).—This comet, discovered by Mr. W. R. Brooks, of Phelps, N. Y., on Aug. 31, is, telescopically, quite a large body with very gradual brightening towards the centre. Observations were prevented by clouds until the evening of Sept. 4th, when it was picked up and lost almost immediately by clouds. A clear sky on the 8th of Sept. permitted observations and the comet was again examined and its place, relative to two stars, was obtained. The comet was large and quite bright and very gradually brighter towards the middle. On Sept. 10th the transit of the comet over a star was observed. On this date I frequently suspected a minute nucleus, and the densest part of the comet was observed to be to the north of the centre.

The following is the position observed on Sept. 8th, 1885:—

Nashville M.T., 1885.	Comet's appt. α.	No. comps.	Comet's appt. δ.	No. comps.	
Sept. $8^d\ 8^h\ 8^m\ 32^s$	$14^h\ 16^m\ 32^s{\cdot}92$	4	$+39^\circ\ 19'\ 26''{\cdot}26$	4	Observed with star *a*.
Sept. 8 8 8 32	14 16 32·63	4	+39 19 27·05	4	Observed with star *b*.

Comparison Stars.

a=W. B. 14^h 278. Mean α 1885·0: $14^h\ 14^m\ 12^s{\cdot}47$; Redn. $+0^s{\cdot}19$. Mean δ 1885·0: $+39^\circ\ 17'\ 42''{\cdot}7$; Redn. $+8''{\cdot}06$.
b=W. B. 14^h 292. Mean α 1885·0: $14^h\ 15^m\ 4^s{\cdot}32$; Redn. $+0^s{\cdot}19$. Mean δ 1885·0: $+39^\circ\ 19'\ 19''{\cdot}4$; Redn. $+8''{\cdot}60$.

Comet *f. a.* $2^m\ 20^s{\cdot}26$. Comet north of *a* $1'\ 35''{\cdot}46$.
Comet *f. b.* $1^m\ 28^s{\cdot}12$. Comet south of *b* $0'\ 0''{\cdot}95$.

Both stars observed at same observations with comet, and therefore the positions refer to the same epoch $=8^d\ 8^h\ 8^m\ 32^s$, Nashville M. T. The observations made with Ring Micrometer on 6-in. Cook Equatoreal. Lat. $+36^\circ\ 8'\ 58''$; Long. $+0^h\ 39\ 1^s$ from Washington.

TRANSIT OF COMET BROOKS (1885 *b*) OVER A BRIGHT STAR.—Upon examining this comet on the evening of Sept. 10th it was seen that the nebulosity would almost, if not centrally, occult a star of probably the ninth magnitude.

Besides the star near the comet, there were two other stars in the field, one of which, some 20″ *n.p.* the first-mentioned star, was almost if not exactly equal to it in magnitude and would therefore serve as an admirable standard for comparisons of the light of the star during occultation. I will call the standard star *a* and the occulted star *b*. Before the encroachment of the comet upon *b* we can take *a*=*b*. At $7^h\ 52^m$ (Nashville mean time) the comet was about 2′ *s. p.* the star. At $8^h\ 6^m$ the star was considerably involved in the body of the comet. At $8^h\ 25^m$ I thought *b* was slightly dimmer than *a*; it was now deeply involved in the comet. $8^h\ 28^m$ *b* was certainly diminished in brightness and was less than *a*. $8^h\ 36^m$ the comet was due south of *b*, power 120; the star was dim, and probably the true conjunction occurred a minute earlier. At $8^h\ 48^m$ the comet's centre had passed somewhat to the *s. f.* of *b*, which was still involved and, I thought, less bright than before immersion; I felt certain it was less than *a*. The brightest part of comet was north of the centre; the star at nearest approach was a few seconds north of the comet's centre and therefore passed through the densest part of the nebulosity. The light of *b* remained steady throughout the occultation; the star-like form was retained without any perceptible blurring. I am certain, after eliminating the effect of a bright background, that the star actually faded slightly during the occultation. The sky was good during most of the observation. The motion of the comet past the star was quite rapid. A power of 60 was employed, except where otherwise stated. Six-inch Cook Equatoreal.

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The Greenwich System of Sympathetic Clocks and the Distribution of Time-signals.

It is proposed in this paper to give a full account of the instruments and methods employed at Greenwich to ascertain the mean time, to have this time exhibited on the various dials throughout the Observatory, and finally for its public distribution. The interval between two successive transits of the Sun over the same meridian is the *solar day*, which day varies in length, first by reason of the unequal apparent motion of the Sun in the ecliptic, and secondly by the non-coincidence of the ecliptic with the equator. A standard measure of time must of necessity be constant, and hence arose the adoption of an imaginary Sun, moving uniformly in the equator with the true Sun's mean motion. The interval between two successive transits of this mean Sun is the *mean solar day*. At the instant of transit of this fictitious Sun, a clock regulated to mean solar time * ought to show $0^h\ 0^m\ 0^s$.

In practice, at least in fixed observatories, the Sun is not used in time-determinations, except on rare occasions. The tables for the Sun being less accurate than star-tables, the transits of certain stars are observed for this purpose. These stars have been regularly observed for upwards of 100 years, and their places are therefore accurately known. The Greenwich clock-star list comprises some 210 such stars. The interval between two successive transits of the First Point of Aries over the same meridian is the *sidereal day*. A clock regulated to sidereal time ought to show $0^h\ 0^m\ 0^s$ when the vernal equinox is on the meridian. Now the Right Ascension of a Star is its hour angle from the First Point of Aries, and therefore the time shown by a sidereal clock, when a certain star is on the meridian, ought to be the right ascension of that star. The difference is the *error* of the sidereal clock Knowing now the true sidereal time, the corresponding mean time can be computed, and thus the *error of the mean-time clock* obtained.

Before following up the actual process, it may be well to describe the principal instruments used. The *standard sidereal clock* being the soul of the whole system, and having some interesting features in its construction, claims something more than a brief notice. The designs for the clock were prepared by Messrs. E. Dent & Co., in conformity with a specification of Sir G. B. Airy, for the adoption of certain new features in respect to—1st, the escapement; 2nd, means for altering compensation without stopping the clock; 3rd, means for altering the clock-rate without stopping it.

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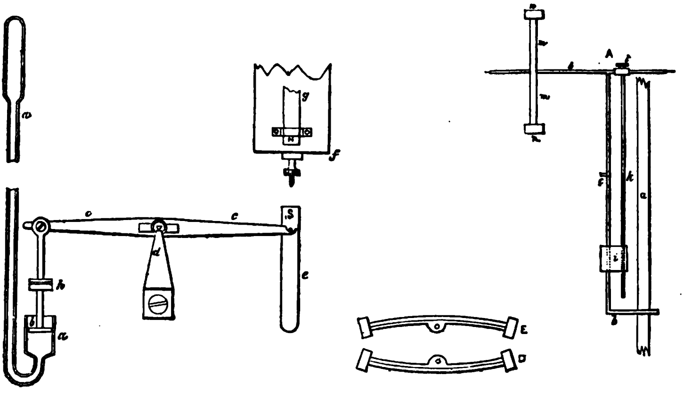
* Since 1885, Jan. 1, all mean-time clocks in the Observatory indicate the number of hours elapsed since midnight, and hence show $12^h\ 0^m\ 0^s$ at the transit of the mean Sun.

The Escapement, invented by Sir G. B. Airy some 40 years previously, is a detached escapement, *i. e.* the pendulum is absolutely free from all connection with the clock-train, except at the time of unlocking and receiving the impulse. Being really an application to a clock of the chronometer escapement, the seconds hand moves forward at every alternate oscillation. Some clocks with short pendulums had been made with this escapement before this time (1868), but none with a seconds pendulum.

Fig. 1.



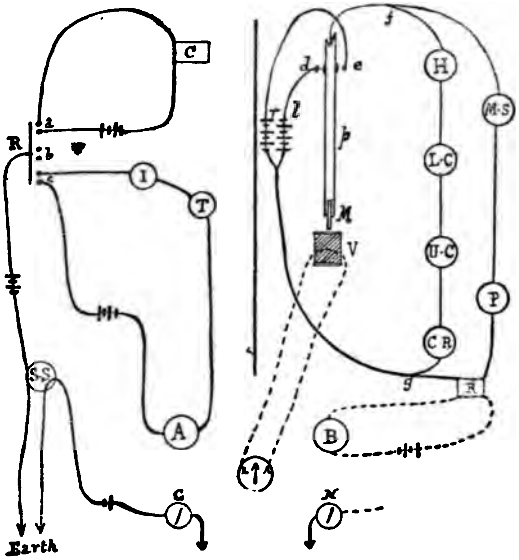
The contrivance for effecting a correction of any outstanding error in the zinc and steel compensation pendulum may be described as follows:—A compensation-bar (*m*, fig. 1) similar to the rim of an ordinary chronometer-balance, but straight at mean temperature, is so adjusted on the axis of the pallets (which theoretically form part of the pendulum) that it may be placed in any position, vertical, horizontal, or at any intermediate angle. This laminated bar carries equal weights (*n*) at its ends. Any increase or diminution of the normal temperature causes the bar to bend and assume the form of a bow, and at the same time gives a motion to the weights, as in E and D. The effect of this motion varies directly as the position of the bar. If the bar is vertical, as in figure, the effect is *nil*, for owing to the centre of motion of the bar coinciding with the centre of its length, the motion of the weights does not affect the centre of oscillation of the pendulum. The maximum effect occurs when the bar is horizontal, as the motion of the weights is then vertical. At intermediate positions the effect varies as the cosine of the angle. If the brass of the rim be upwards (E), the effect of a rise in temperature is to *lower* the weights and make the clock *gain*. The temperature of the magnetic basement where the clock is located, being practically uniform, and the compensation of the pendulum being so exact, this ingenious apparatus is not brought into action.

To alter the rate without stopping the pendulum, a small weight (*i*) (about ½ oz.) is made to slide up and down on the crutch, *b* in fig. 1, by means of a screw (*k*) which terminates in a milled head (*e*). This milled head being at the centre of motion of the crutch, it can be turned without causing disturbance. An index and division on the milled head give the means of noting the exact distance the weight is moved.

It was originally intended to have a mercurial pendulum, but owing to the excellent performance of Messrs. E. Dent & Co.'s new zinc and steel pendulums, as fitted to the Transit of Venus clocks, it was decided to apply a similar pendulum to the standard. These pendulums are better fitted for travelling than mercurial pendulums, and the bob being suspended at its centre of gravity instead of at its top or bottom, its action on the compensation is annulled. The results have proved the wisdom of this change.

The clock was brought into use early in 1871, and its rates were extremely good, although subject to small variations clearly traceable to fluctuations in the density of the atmosphere. Sir George Airy devised a means for compensating this barometric inequality which is deserving of notice. The principle is the same as that daily made use of in adjusting the mean solar clock, and devised some years previously by Sir George (see 'Mean Solar Clock'). Two bar-magnets are fastened vertically to the pendulum-bob, *f* in fig. 1, one in front, the other behind it. About 4 inches below these is a horseshoe magnet, *e*. The poles of the three magnets are so arranged that there is attraction between the pendulum magnets and the horseshoe below. Now it is obvious that if the horseshoe were a fixture, the effect would be constant, except for such fluctuations as are caused by variation in magnetic intensity. But it is movable, being carried on one end of a lever, *c*, whose other end is attached to an iron float, *b*, in a syphon barometer placed in the clock-case. The weight, *h*, is a counterpoise for the magnet, *e*. As therefore the level of the mercury varies according to the change of atmospheric pressure, so does the distance between the horseshoe magnet and the magnets on the pendulum-bob. The attractive force on the latter being vertically downwards, the effect is to assist gravity. With a low barometer the pendulum gained on its rate; this is counteracted by the magnets being more separated. The contrivance seems to have acted satisfactorily, and has been adopted in several foreign observatories. Originally the electric circuit in connection with the sidereal standard was closed every second by the pendulum, but it was found to cause fluctuation in the clock's rate, and the contact is now made by the clock-train. The ruby pin, *c*, on the crutch was used for lifting the springs into contact. At each beat (which, as we have seen above, is every alternate second) a wheel of 30 teeth on the arbor of the seconds hand closes a galvanic circuit (fig. 2) and so drives the sidereal relay R on the Superintendent's desk in the lower computing room. This contact is made when the pendulum is quite free from the train after the impulse has been given. The inconveniences arising from the use of the alternate second beat have been found to be more than balanced by the steady rate of the clock. The current employed should be a weak one, so as to cause as little corrosion as possible in the contacts. Another circuit is closed by the clock at the 0 second every minute, and deflects the needle of the galvanometer G near the sidereal chronometer A. By means of this it is ascertained that the latter shows the same second as the standard. This completes the work done by the S.S. *directly*. The sidereal relay R, by closing three pair of springs, performs the remaining work. These springs *a*, *b*, and *c* are shown in fig. 2.

Fig. 2.



- *a* closes a circuit in connection with the chronograph C.
- *b* completes the circuit for controlling a chronometer on the eye-end of the S.E. equatoreal.
- *c* closes the circuit in which are the sidereal chronometer A, the tapper T, and the 10-inch clock I; the two latter being in the S.E. dome.

The Chronograph consists of a brass cylinder suspended by its axis on friction rollers, and kept revolving by a clock-work governed by a conical pendulum. The motive power is a weight. A spindle in the clock-train also gives motion to two screws, parallel to the whole length of the cylinder. By this motion a frame carrying two electro-magnets is made to travel along the cylinder, at the rate of $\frac{1}{10}$ inch in two sidereal minutes. To each magnet is attached a lever with an armature on one end, a pricker on the other, so that, when an electric current passes through the coil, the magnet pulls the armature, and thus causing the pricker to make a puncture on a sheet of paper folded tightly round the barrel. One magnet receives a current every alternate second from the sidereal relay R, making the pricker record a continuous spiral of punctures two seconds apart. Through the other magnet a current is made to pass by the observer each time a star transits one of the vertical wires in the transit telescope; a corresponding puncture is made on the paper. Thus there are two sets of punctures, one recording the transits, the other registering the beats of the Sidereal Standard Clock. To obtain absolute sidereal standard times, signals are sent from the clock Hardy, whose error on the S.S. is known. The cylinder revolves once in two sidereal minutes; the distance between the clock-taps is ·6 inch.

The Mean Solar Clock is by Shepherd, and is essentially a galvanic clock. The motive power is derived as follows:—When the pendulum *p*, fig. 2, swings to the right it closes a galvanic circuit passing through the coils of two electro-magnets (not shown in the fig.), causing them to attract an armature on one end of a lever, which in turn lifts another smaller lever with a weight attached; a small detent holds this weighted lever until the pendulum, in swinging to the left, lifts the detent and allows the weight to fall and impinge on the pendulum. An impulse is therefore given to the pendulum each time it swings to the left, *i. e.* at each double vibration. When the pendulum is once set in motion, these impulses are sufficient to maintain it. The mean solar circuits are shown in fig. 2. Two batteries *r* and *l* are used, *r* being worked when the pendulum swings to the right, and *l* when the pendulum swings to the left, making contact at *d*. These batteries are thus used every alternate second, and it will be seen from the arrangement of the plates that the two currents are of different character. The form of battery found most useful is that known as the Walker, consisting of one fluid, dilute sulphuric acid, and two plates, one zinc, the other graphite. The mechanism of the clocks is the same in all; a permanent bar-magnet is poised on an axis and made to oscillate, in consequence of its ends being alternately attracted by electro-magnets placed below, one set being in the right, the other in the left clock circuit. This oscillating movement is converted into a rotary motion by the action of anchor-pallets on a toothed wheel fixed to the arbor of the seconds hand, this latter being connected with the minute and hour hands by a proper train of wheels.

These clocks controlled by the one pendulum are :—

M.S. Dial of the mean solar clock immediately above the pendulum.
P. Public clock near the gate.
H. Clock in the hall of the Astronomer Royal's house.
L.C. Lower computing room clock.
U.C. Upper computing room clock.
C.R. Chronometer room clock used for rating the chronometers for the Royal Navy.
R. Is the seconds relay used to drive the small mean-time chronometer B on the desk of the Superintendent of the Time Department, and near A the sidereal chronometer. The circuit for B is shown by the dotted lines.

The circuit is split at *f* and *g*, in order to minimize the effect of a failure in the wires.

Imagine now that the pendulum swings to the right, and makes contact at *e*, the current may be considered as passing in the direction $r, e, f, \frac{H}{P}, g, r$. The next second contact is made at *d*, and the current passes $l, d, f, \frac{H}{P}, g, l$. Thus a current passes through one or other of the sets of coils every second, causing all the clocks to beat in unison with M.S. The needle of the galvanometer N, in connection with M.S., is made to deflect at the 0^s of every minute, and thus enabling the second of B to be checked. There are two adjustments of the mean solar pendulum :—

1. Adjustment of rate.
2. Correction of error.

The first is secured by means of a magnet, M (fig. 2), attached to the pendulum, and swinging under a bundle of soft iron wires fixed to the clock-case. Moving the wires nearer the magnet causes the pendulum to gain on its rate, and *vice versâ*. The clock's error is corrected as follows :—V is a coil in a galvanic circuit, and so placed that the N pole of the small magnet M, referred to above, passes over the hollow core. On the Superintendent's desk is a commutator, by means of which alternate currents can be made to pass through the coil. Thus suppose the handle (fig. 2), turned to A, the upper end of the coil becomes a S. pole, and attracts the N. pole of the magnet M, and causes the pendulum to accelerate ; it, in fact, assists the action of gravity. Turning the handle to R causes a retardation. The strength of the battery is arranged to allow a correction of about $\frac{1}{10}$ second to be made in one minute. A too rapid change might cause the sympathetic clocks to break away from control. T. Lewis.

[To be continued.]

*The Story of the Heavens**.

Our first feeling on meeting yet another popular work on astronomy, and that 551 pages in length, was one of dismay rather than welcome. What could there be said that had not been said, and well said, before?

It must be frankly confessed that Dr. Ball soon overcame this our not unnatural prejudice. Every page of his book shows the style and method of an experienced writer and lecturer; in not a few instances Dr. Ball has seen his way to improve on the methods of his predecessors, or, failing that, has at least succeeded in putting old truths into language that reads easily and fresh. Not only are points made clear which are too frequently left obscure or misstated, but in several cases information is given that even our best writers have omitted. How many readers, for instance, will learn here for the first time that meteorites are not always, nor even usually, composed entirely of iron?

It must be perfectly understood, as, indeed, the title would suggest, that Dr. Ball has not been writing here for the advanced student, but for the beginner, or the general reader; and his purpose, which is for the most part admirably carried out, is to supply a popular account of the various members of the solar system, and of the leading facts of the stellar universe, and at the same time to explain in simple language, without the aid of mathematics, as many of the more difficult points of astronomy as seem to lend themselves to such treatment. Particular attention is also paid to the revelations of the spectroscope, and the theories of comets and meteorites.

It would be impossible for us, in our scanty space, to notice seriatim the different portions of the book; but two call for at least a passing mention, as they deal with subjects which Dr. Ball has made specially his own. These are Chapters XXI. and XXVII.; the former of these dealing with the Distances of the Stars, the latter with the Tides. Chap. XXI. commences with describing Herschel's method of Soundings and the principle upon which he worked. (It should have been pointed out, however, that Herschel's original hypothesis was defective, and was recognized by himself to be such in later years.) Annual Parallax is next explained, and the labours of Bessel and Struve referred to. The extreme minuteness of the apparent displacements of star is next insisted on, and the wonderful star Groombridge 1830, with its velocity of 200 miles a second, is discussed at some length. Lastly, the discovery of the motion of the solar system through space is dwelt upon, though here again a word of caution might have fitly been dropped as to the trustworthiness of our present results. The chapter on the Tides is one of the most interesting in the entire book. Dr. Ball traces the various steps in the process by which he believes the Earth and Moon have arrived at their present relations to each other, from the time when the Moon was first separated from the Earth and revolved round her at a very short distance. Dr. Ball's views have not escaped criticism; but the attractive and interesting manner in which he has here set them forth cannot be denied.

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* 'The Story of the Heavens.' By Robert Stawell Ball, LL.D., Royal Astronomer of Ireland. With coloured Plates and numerous Illustrations. London: Cassell & Co.

The book is well illustrated; the 16 plates, though nearly all are now well known, are amongst our best astronomical illustrations. The woodcuts are most of them new, and are clear and well executed. The series of diagrams for teaching the positions of the principal constellations deserve especial commendation. The diagram showing the position of the spot-zones is inaccurate. Spots of smaller latitude than 10° are quite common, and in the years immediately preceding the sun-spot minimum the *majority* of spots lie on a narrow equatoreal belt, few attaining so high a latitude as 10°. The diagram of the Transit of Venus on p. 153 is not to scale; but on the whole the woodcuts are perfectly correct. The type is clear, the paper good; and the only drawback is the folding, which, so far as our volume at least is concerned, is carelessly done.

E. W. Maunder.

The Liverpool Astronomical Society.

The Opening Meeting of the Session 1885–86 was held on Tuesday, Oct. 13, 1885, in the Association Hall, Mount Pleasant, Mr. Isaac Roberts, F.R.A.S., F.G.S., President, in the Chair. There was a good attendance. Among those present were Messrs. R. C. Johnson, F.R.A.S., W. H. Sharp, H. Thomas, and J. W. Appleton, F.R.A.S. (Hon. Sec.); besides Mr. W. H. Finlay, B.A., F.R.A.S., of the Royal Observatory, Cape of Good Hope, who is on a visit to this country.

In his opening remarks the President said that the Society during the last two years had been very successful; the number of Members now exceeded 200, many of whom were non-resident in Liverpool. The Society included many hard and careful workers in Astronomy, and they might congratulate themselves that the Society was now well known thoughout the world.

In the course of his address he said that one of the chief astronomical events of the year was the appearance of a star in the nucleus of the nebula in Andromeda, and that on Sept. 3, immediately after receiving the Dun-Echt circular, he examined the nebula with his 7-inch refractor by Cooke, and photographed it with his 20-inch reflector by Grubb. The refractor showed the star-like nucleus very clearly and sharp. A Maclean spectroscope showed the well-known dull continuous spectrum without any bright lines.

Seven photographs were taken on Sept. 3, with exposures of one, two, four, eight, sixteen, thirty-two, and sixty respectively.

No trace of the nebula was to be seen on the plate which was exposed for one minute. On the plate which was exposed for two minutes about twenty stars appeared, but the nebula was uncertain. On that exposed for four minutes 37 stars were seen, and the centre of the nebula appeared like one of them, with a faint shading surrounding it. On the plate exposed for 16 minutes the star and nucleus were clearly shown, and they were *not coincident.* On those exposed for 32 and 60 minutes respectively, the star and nucleus were very clearly shown. On the 60-minute plate about 53 stars could be traced within a circle of 40′ of arc in diameter with the *Nova* star as the centre. The whole nebula could be traced only to about 3′ of arc in diameter, and was a nearly circular patch of light with an undefined boundary, the star being a little distance (about its own apparent diameter) to the south-west side of the nucleus.

Eleven photographs of the nebula had been taken between Sept. 3 and 30, and they all confirmed the description here given.

The drawings of this nebula by Argelander, by Bond, by Mr. Espin (late President), and by Mr. Common differed from each other. (They were drawn on the blackboard to *a uniform* scale so that they could be compared.)

Argelander's resembled some of the well-known forms of Diatomaceæ, and had a longitudinal diameter of about 50′ and transverse diameter of 15′. Bond's had a different form with prominent dark and white markings, the extreme length being 120′ and the breadth 90′.

Mr. Espin's drawing was given in last session's Journal of this Society, page 86, and resembled Argelander's except that it was nearly straight on its north-western side. The length of the nebula was 65′, and 10′ in breadth. Mr. Espin stated that this drawing was made from three photographs which he took with a camera and lens of (he understood) 4½ inches aperture, and exposure of 60 minutes.

Mr. Common on the 1st of this month published in 'Nature' a drawing of the nebula which he took with his 3-feet reflector on Aug. 16 last year. The nebula appeared as a slightly oval patch of light, 4′ 35″ in length, by 3′ 20″ in breadth, and the drawing corresponded well with his own (Mr. Roberts') photographs.

That drawings made by eye observations of nebulæ differed widely from each other there was ample experience, but that *photographs* should bring about a similar state of bewilderment was quite a new and disappointing experience. It had been expected that all photographs would consistently show more or less of the details of a nebula according to the conditions of the atmosphere, the sensitiveness of the plates, the time of exposure, the focussing of the instrument, the development of the image, &c., but such differences as were here exhibited had not been anticipated. In order to account for them it must be assumed, *firstly*, that a photographic lens of 4½ inches aperture was better as a photographing instrument than either a 20-inch or a 3-feet reflector; but his (Mr. Roberts') experience, after many experiments with lenses varying between $\frac{3}{8}$ inch and 5 inches aperture, was that they did not photograph so many stars under equal conditions as the 20-inch reflector. Mr. Espin's drawing also proved this fact, for within a circle of 40′ diameter, described from a point near the nucleus as a centre, he showed on his drawing 14 stars. The 20-inch reflector showed 53 stars within the same limit, whilst Mr. Common's 3-feet reflector showed 124 stars without a magnifier. *Secondly*, it must be assumed that the nebula had changed between Sept. 17, 1884, and Sept. 3, 1885. Mr. Common's photograph was taken on Aug. 16, 1884, and Mr. Roberts's photographs were taken between Sept. 3 and 30 this year, and, with the exception of the new star in the nucleus, they are very similar, and did not indicate a shortening of the nebula from 65′ as Mr. Espin showed it, to 4′ 35″ as Mr. Common's and as Mr. Roberts's photographs showed it.

If Mr. Espin had not misinterpreted his photographs the changes were of a startling character, and the questions involved were of great importance to astronomers. His photographs ought to be submitted to some competent person, in order to confirm his drawings and observations as given in the Society's Journal of last session, and if an error had been committed it was essential that it should be corrected.

Mr. W. H. St. Q. Gage also contributed a paper on the same subject, ably arguing that the new star was in the nebula itself. The following are the conclusions at which he arrived:—

1. That it was highly probable—nay, almost certain—that the *Nova* was in the nebula.
2. If optical effect was of any value, that it was *far down* in the nebula.
3. That it was 1′ preceding the present nucleus, and slightly south of it.
4. That its colour was bright orange, though its variation was probable.
5. As regarded the nebula itself, that its nucleus had brightened, and that the whole object was brighter than formerly (Mr. Gage had rated it at about $5\frac{1}{2}$ magnitude star by naked eye on Oct. 3).
6. That a great change had taken place in the nebula, the exact nature of which is uncertain, and that the so-called "star" differed in many essential respects from other objects generally called by that name.

Mr. R. C. Johnson remarked that the nebula was singularly non-actinic, and urged the necessity that its shape and size be exactly ascertained, for if it changed so much as had been supposed, many important questions were suggested respecting it.

Mr. Finlay gave an interesting account of the photographic charts of the sky that they were taking at the Cape of Good Hope. This idea had been taken up in Paris, as well as by the L.A.S. They used at the Cape a 6-inch lens with 5-feet focal length; but had ordered a 9-inch lens which would soon be ready.

The whole Southern Hemisphere would be divided into sixty squares, each one overlapping another adjoining it. Two plates were taken of each picture, so as to avoid any mistake. It was expected that the work would take from three to four years, and would comprise 1000 pictures. The length of exposure was one hour. So far the work had been very successful, the images being very good and round, and the doubles very clear in a magnifying-glass; but the work of measuring had not yet been begun. The electric light had been used for the microscopes.

A paper by Mr. Rand Capron on "Celestial Photography" was read, one by Mr. Fleming giving a short account of a lunar halo and paraselenæ visible at Kirkwall, and other interesting and valuable papers were contributed.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Statements and Restatements concerning Jovian Markings.

GENTLEMEN,—

So long a time must elapse before my note can appear, that my impulse on reading Mr. Denning's communication ('Observatory,' No. 101) was to say nothing more on Jovian phenomena; but I find myself quite unwilling to remain subject to some misapprehensions which his notice of my former communications may induce.

1. In view of the very fine seeing on May 8 and 10, it does indeed seem *strange* that I did not see "the faint narrow belt abruptly bent down" to the Red Spot—"the narrow and very definite vein of dark matter crossing the white zone outlying the spot," which Mr. Denning says he saw on May 9. Surely the size of our objective is not so great as to make it liable to the faults ascribed to large instruments, such as the great telescopes at Chicago, Princeton, and Washington. Possibly if I had examined Jupiter on July 9, I should have seen this long-sought marking. Or does it imply too great credulity to think the phenomenon may be variable? I propose, when opportunity again occurs, to keep watch for this phenomenon; and when I see it, I shall not be slow to acknowledge it.

2. I am not willing that the sketch accompanying my communication ('Observatory,' No. 100) should be taken as a *full representation* of the various minute details seen at the date on the disk of Jupiter. I did not suppose any one would so regard it. That sketch had one main object—to show the Red Spot and its immediate environment. Only the main features of the planet were expected to be shown, in addition. It is not fair, then, to conclude that I have not seen the darkened following end of the spot, nor the longitudinal division of the belt, on the simple showing of a

hasty sketch, made for a *special purpose* by another hand. Still less is it right to infer, from this *supposed failure*, my inability to see the faint belt "abruptly bent." I have myself noted several details about the Red Spot, of which, as yet, I have seen no notice; but from this I do not feel at liberty to conclude that no one but myself has seen them.

3. The remark in my communication about the proper motion of the Red Spot was *incidental*. I do not hold it as a *final article* of scientific faith. But as Mr. Denning asks if my opinion is founded on a statement quoted from the 'Observatory' for January 1879, I answer, at once, *it is not*. Wishing to controvert the opinions of no one, I will simply say the opinion seems to me not *unreasonable* from the following considerations:—

(1) The evident *agitation* of the planet's surface about that time implies a physical cause adequate to so unexpected a result.

(2) The great *difficulty*, or rather *impossibility*, of reconciling the early observations in the absence of *proper*, or at least *irregular motion*.

(3) As shown ('Observatory,' No. 30), I examined Jupiter very frequently in the latter part of 1878, when, to correspond with previous observations, the spot should have been visible, and sometimes near the centre of the disk. From the testimony of Messrs. Dennett, Trouvelot, and Niesten, I am assured it was not *invisible*. Atmospheric conditions do not account for my failure to see it. *Proper motion* or *irregularity* of motion seems to me to offer the best explanation.

4. I am unwilling that Mr. Denning or any one else should construe the quotation made from 'Observatory,' No. 21, as calling in question the reality of the proper motion of the white spots relatively to the Red Spot. On the contrary, I noted this motion very frequently as early as 1878 and 1879. My notebooks contain numerous references to it ('Observatory,' No. 34). Moreover, whatever incredulity others may entertain, I honour Mr. Denning and Prof. Hough with my full confidence in their continuous and painstaking determination of the relative rotation of one of these spots.

5. If, in the quotation made, I was so ignorant as to confound an apparent proper motion with the surface displacement due to axial rotation, then the statement justly deserves the unsparing criticism it receives from Mr. Denning. The planetary rotation may have led me to *over-estimate* the apparent proper motion; but making a large *subtraction* on this account, there remained still a *very great change* in the relative positions of the two spots at the *beginning* and *end* of observation ('Observatory,' No. 30, p. 178). Still, from later experience gained in observing the ever-varying condition of Jupiter, I should not *now* call this "proper motion." Since that date (July 1878) I have witnessed phenomena which convince me that there are sometimes changes in the *intensity* and *position* of bright centres on the Jovian disk, occurring

even under the eye of the observer or during observations lasting through an hour or two, which give the *appearance* of proper motion when none exists. Whether the envelope of Jupiter consists of stratified clouds raised in his atmosphere by the Sun's action or his own internal heat, or a series of semi-rigid shells of metallic vapours, we may conceive of beams of light from the Sun or from the planet pouring through openings in the strata, and lighting up and intensifying small areas in the visible surface. The result might be accomplished by the progressive transmission of light through varying media, or it might be a part of an auroral display. I may refer particularly to an instance which I described in the 'Kansas City Review' for February 1882. The observation was made with all the spots under the micrometer-threads, and while observing the transit of the Red Spot, Dec. 23, 1881. As I expect to republish an account of this observation in a small volume of observations soon to be issued, I will not encumber space with the details now. It is sufficient to state that the *apparent displacement*, as measured by the micrometer, was more than 4″ in an hour. Still I think there was no *motion of translation* in the Jovian atmosphere, but simply a *progressive motion* of light through changing media, intensifying in succession *different centres*; otherwise I should have to believe that a motion of translation can occur in the Jovian atmosphere at the rate of 10,000 miles an hour. Very respectfully yours,

C. W. Pritchett.

Morrison Observatory, 1885, Sept. 28.

Photographing the Corona.

Gentlemen,—

Mr. W. H. Pickering having courteously sent me a copy of 'Science' (August 14) containing a letter entitled "An attempt to Photograph the Solar Corona without an Eclipse," may I ask you to insert the few lines which follow in the next number of your Journal? Passing by all those points which are covered directly or indirectly by my reply to Mr. Pickering's first letter ('Science,' April 3), I find only two matters which I consider it necessary to notice.

(1) Mr. Pickering says:—"The inferiority of the best gelatine plates to the human eye in this respect (small differences of light) is very readily shown by an attempt to photograph distant mountains." He then goes on to say:—"Another illustration of the same thing is the impossibility of photographing the Moon in the daytime when the Sun is high above the horizon. Although the Moon may be perfectly distinct to the eye, the negative shows no trace of it." To your scientific readers the reasons will readily suggest themselves why in the case of the Moon in the daytime, at some angular distance from the Sun, the eye has an advantage over the plate, while in the case of the corona the plate has a great advantage over the eye. Apart from any such considerations, as a matter of fact, *there is no difficulty in photographing the Moon*

at noonday. Yesterday I took, with the apparatus used on the corona, four negatives on bromide plates (Edwards') between 11.30 A.M. and noon, in full sunshine. On all the plates the Moon is very distinct and well defined. The Moon at noonday, unless too near the Sun, is an easier object to photograph than the Corona. It is obvious therefore that photographic methods, which are not delicate enough for the Moon, must utterly fail if applied to an object still more difficult, as the corona undoubtedly is, at ordinary elevations.

If Mr. Pickering's statement of the "impossibility" of photographing the Moon under the conditions already named rests upon his own experiments, some light may come upon a point which has occasioned me surprise, namely, that Mr. Pickering does not appear to get upon his plates the defects of his own apparatus; for example, those of the position of his shutter and those of his spectacle lens. In some experiments I made with a shutter similarly placed, strong diffraction-effects appeared on the plates —effects stronger than any photographic action which could be supposed to be due to the corona.

(2) With regard to Mr. Pickering's experiments, I would point out that the conclusion to which they lead him,—namely, "It therefore seems that even in the clearest weather the reflected light of the atmosphere is 300 times too strong to obtain the faintest visible image of the true coronal rays,"—appears to me to be irreconcilable with the direct observations of Prof. Langley and others of the planets Mercury and Venus as black disks before they reach the Sun. Prof. Young says:—"Of course this implies behind the planet a background (of corona) of *sensible* brightness in comparison with the illumination of our atmosphere" ('The Sun,' p. 229).

I trust that Mr. Pickering will not think me wanting in courtesy if I should prefer not to take notice of any further communications he may make on this subject. The Bakerian Lecture, read recently before the Royal Society, in which I have discussed some of the points named above more fully, will be in print in a few weeks. The photographic method is now being tried at the Cape of Good Hope, under the scientific conditions I have pointed out as essential, by Mr. Ray Woods, under the able superintendence of Dr. Gill, F.R.S.

Upper Tulse Hill, London, S.W. WILLIAM HUGGINS.
1885, October 2.

Bayer's Nomenclature.—Variability of α Draconis.

GENTLEMEN,—

In your number for the present month (p. 340) Mr. Denning refers to my remark from Arago that Bayer, although he made no attempt, as has been often supposed, to determine the comparative brightnesses of the principal stars in the different constellations and to designate them accordingly by the alphabetical sequence of the Greek letters, yet did uniformly assign the letter α to that star which was then considered to be the brightest in each constellation. Mr. Denning says :—" β Geminorum is now considerably brighter than α. γ Draconis is more than one magnitude brighter than α and ζ ; η and β of that constellation also exceed α. β Herculis is brighter than α, &c. Are these and other instances to be referred to actual variation in the light of the stars, or to looseness in applying the original nomenclature?"

Now before speaking of the general scheme as shown in other instances, let me point out that the case of the stars in Draco is altogether exceptional. It is singular, not plural as Mr. Denning's language would seem to imply. For Bayer reckons one star, and one star only, in Draco as being so bright as the second magnitude; that star is α. The next ten, from β to λ, he rates of the third magnitude, and designates them according to his usual system, with no reference to their comparative magnitude. Now in this case we have in all probability an instance of real change of brightness of very long period. For while the ancient astronomers reckoned α Draconis of the third magnitude, Tycho, Hevelius, and Flamsteed estimated it of the second, and (as is well known) it is now considerably below the third, and fainter than several other stars in the constellation. The fact of change appears to have been first noticed by the elder Herschel, who says, in his paper " On the Proper Motion of the Sun and Solar System," in the 'Philosophical Transactions' for 1783, vol. lxxiii. pt. 2, p. 254 :—" α Draconis is so much less than β, which is set down as a smaller star in Flamsteed's catalogue, that the change of magnitude cannot be doubted." On this E. Pigott remarks (Phil. Trans. for 1786, vol. lxxvi. pt. 1, p. 208) :—" I am of Mr. Herschel's opinion that it is highly probable this star is variable. Bradley, Flamsteed, &c. mark it of the second magnitude; at present it is only of a bright fourth. I have frequently examined it since October 1782, without perceiving the least change, being constantly rather less than ι Draconis, equal to δ Ursæ Majoris, and rather brighter than κ Draconis."

Mr. Gore refers to these facts in his recent 'Catalogue of Suspected Variable Stars, with notes and observations.' Mr. Chambers does not include α Draconis in the list of " Stars probably variable " in his 'Handbook;' but then he is careful to remind us that no attempt was made to render this list exhaustive.

Now with regard to the other cases mentioned or referred to by Mr. Denning. Bayer nowhere states positively what he did, and we are left to infer it from an examination of the magnitudes as divided into different classes or scales in his 'Explicatio.' As I said in my letter in your July number, it is quite clear from this (as was pointed out by Argelander) that whilst each class of magnitude consists of stars fainter than those in the preceding class, no attempt was made to arrange the stars *in each class*, designated by the letters of the Greek alphabet in sequence, according to their magnitudes, but they were rather made to follow the form of the figure supposed to be represented by the constellation. Arago, however, pointed out an evident desire on Bayer's part to call the single star in each constellation which was considered to be the brightest of all by the letter α. In this he seems to be right: but I do not think the rule was carried out strictly without several exceptions. Thus in the case of Gemini, Bayer seems to have thought that α and β were equally conspicuous stars, each in the head of one of the twins, and he probably lettered them in their order of right ascension. In Hercules (and a few other cases), finding but little difference between several of the brightest stars, he seems to have fallen back upon his ordinary practice of giving precedence to a star in the head of the imaginary figure or some other consideration tending to symmetry of form. Instances must occur to many in which weight has been given to the last consideration. Upon the whole, I think we may say that the rule referred to by Arago was only observed when the brightest star in the constellation was very decidedly the brightest; and that it would be unsafe to infer in any case change of magnitude from non-correspondence of the heavens with Bayer's letters.

Yours faithfully,
W. T. LYNN.

Blackheath, 1885, Oct. 2.

The Pyramids and the Earth's Axis.

GENTLEMEN,—

In his interesting and valuable work on the 'Pyramids and Temples of Gizeh,' Mr. W. M. Flinders Petrie says that by the orientation of the Great Pyramid the place of the pole at the epoch of its building would be 5′ 40″ ± 10″ west of north. He refers to the latitude observations at Greenwich and Pulkova as evidencing a change in latitude of about 1″ per century, and thinks that observations show "a shift of the Earth's axis" to an extent that would account for the discrepancy in the Pyramid orientation, suggesting also a hypothesis (as helping to form an idea of the cause of this change) of a ring of water circulating round the Earth, across the poles, at 1 mile per hour, and only 4 square miles in section. He adds, "thus the apparent change in the axis of rotation shown by the orientation of the Pyramids is of the same order as a change actually observed. It is also far within the changes likely to be produced by known causes, and the uniform deviation is otherwise unaccountable in its origin. Hence it appears that it may be legitimately accepted as a determination of a factor, which is of the highest interest, and which is most difficult to observe in any ordinary period." And in a note he suggests careful re-determinations of the meridians fixed in the beginning of the Ordnance Survey, as (according to the pyramids) a change of 5″ might be expected in their azimuths.

Now, is it not generally held that the idea of some respecting a possible continuous change of latitude is at all events hitherto quite unproved? It may interest others besides myself to learn the opinion of the astronomers at the Royal Observatory regarding the small changes in the adopted co-latitude which are made almost every year. Would considerations of refraction, instrumental errors, and possibly local movements of the ground suffice to account for these without any hypothesis of the shifting of the Earth's axis? But even on this supposition it is difficult to see how it could affect determinations of *azimuth*. Mr. Petrie thinks the setting out of the orientation of the sides might have been done without much difficulty, by observations of the pole star at its transits at the elongations behind a long plumb-line. At a distance of 100 feet, 15″ of angle would be about $\frac{1}{10}$ inch, and therefore quite perceptible (p. 84). He finds the orientation difference so much larger than the errors of setting out the form (which average 12″) that such a divergence might be wondered at. Others may consider that it is not by any means greater than might have been expected, or even so great as might have been looked for in the absence of the telescope and instruments of precision. Any remarks on the subject that may occur to you will be very acceptable.

The hypothesis in question occupies but one page (p. 42, 2nd ed., 1885) of this truly admirable work, and doubtless the able and laborious author would be glad to be corrected if it be untenable.

Teignmouth, Devon.
1885, Sept. 21.

Yours faithfully,
GEORGE J. WALKER.

P.S.—Mr. Petrie finds a confirmation of his view in the entrance passage, which he thinks was probably set out by a different observation, and the azimuth of which nearly agrees with the above-mentioned divergence.

[The question of secular variations of latitude has been ably discussed by Prof. Asaph Hall ('Observatory,' No. 96, pp. 113–117). The conclusion at which he arrives (and with which we quite agree) is that an astronomer of experience would be cautious in ascribing the apparent changes of latitude as observed at Pulkowa to an actual change, and that, in the case of Greenwich, there is no evidence of a secular change of latitude. The scheme of observations sketched out in Prof. Hall's paper will no doubt be sufficient to decide this interesting and important question.—EDITORS.]

Hyginus N.

GENTLEMEN,—

I trust I may be allowed a little space to make a remark touching Mr. Rand Capron's letter in your September number.

My Beer and Mädler's map is one of the original issue, dated 1834. I have carefully compared Mr. Wesley's tracing with the same region in my map, with which it closely agrees, showing the elongated elliptical marking, in the approximate position of Hyginus N, very plainly. The shaded background in B. and M.'s map is entirely composed of minute dots, which do not contribute to the clearness of the delineations.

Yours faithfully,
GEORGE HUNT.

Hopefield, West Dulwich,
1885, Oct. 13.

The November Meteors.

GENTLEMEN,—

The near approach of these meteors may form my excuse for offering a hypothesis as to their nature, which, though largely borrowed from Mr. Proctor, may be worth stating in the form in which I now send it.

It is that there has been at times not very remote (for comets of the class of Tempel's are probably not very long-lived) a tremendous volcanic disturbance on the surface of Uranus, which led to the ejection both of the comet and of the flight of meteors. That a volcano of sufficient power is capable of propelling matter from a planet into space must, I think, be conceded. Dr. Ball, the Royal Astronomer of Ireland, thinks that many (if not all) of the meteors which now fall to the earth were originally ejected from it by the more powerful volcanic action which took place in former ages. We have only to increase the present effects of volcanic action to effect the desired ejection.

At the commencement of an eruption a great quantity of gas is usually expelled from the crater of the volcano. This gas would (if propelled with sufficient violence) form a comet like that of Tempel; for the small mass of a comet compared with its size, indicates that it is chiefly composed of gases in a state of great tenuity. But after the gas has been propelled from the crater of a volcano, there usually follows a shower of stones; and if this took place in the case of the great eruption on Uranus, a shower of volcanic stones would follow the course of the comet. They would not indeed probably follow the same precise course, for the propelling force would vary more or less both in direction and velocity. But, roughly speaking, they would follow the track of the comet, though several of them might deviate from it considerably.

It has never been proved, I think, either in this case or in that of any of the comets whose origin appears to be connected with the other exterior planets, that the comet and the flight of meteoric stones are identical. It has only been proved that they follow the same track. This would be sufficiently accounted for by supposing that both originated in the same great volcanic eruption.

But we have to a certain extent direct evidence on the subject. Meteoric stones have on many occasions fallen to the earth and the volcanic appearance of these stones has been a subject of general remark. They do not at all resemble aggregations formed by the condensation of highly rarefied gases. But it may be asked, Are any of these meteoric stones genuine Leonids? I regret that I am unable to answer. But I think there can be little doubt that on more than one occasion the actual fall of Leonids has been observed, and I have not heard of any remarkable peculiarity in the stones which thus fell. I hope, however, that some astronomer may be able to set the question at rest next month, by capturing a Leonid or an Andromede whose fall has been noticed. I may add that a modification of the same volcanic action would probably afford an explanation of the ring-system of Saturn.

Dublin, 1885, Oct. 10. W. H. S. MONCK.

Recent Measures of a Centauri.

GENTLEMEN,—

In the 'Observatory' for October last were published measures of this interesting binary made by me in July 1884. As they were of some use in verifying the elements of the orbit computed by Mr. Downing, I am encouraged to send the results of four sets of measures obtained here on July 11, 27, 31, and August 3 of the present year. These were taken, as before, with the 4½-inch equatoreal, and within two hours of the upper culmination, the number of measures east and west being equal for each date. On July 11 the number on each side of the meridian was four for position-angle and five for distance, and the distance was deduced from the position-angle and measured differences of declination. On July 27 and 31 the observations for position-angle and distance were equal on each side of the meridian, and the distance was obtained directly by double measures in the usual way. On August 3 observations for position-angle only were made. In every instance the steadiness and definition were much more satisfactory before than after culmination. Finally, it will be seen that the tendency in July 1884 to make the position-angle greater on the western side of the meridian is also exhibited in the results of the present year.

Epoch.	Eastern Measures.		Western Measures.		No. of Measures.
	Position-angle.	Distance.	Position-angle.	Distance.	
1885·527	198°·6	13″·47	201°·7	14″·03	4·5
·571	199·8	14·40	202·0	14·17	7
·582	200·8	14·08	201·2	14·17	10
·590	201·1	....	201·2	....	10

Windsor, N. S. Wales,
1885, Sept. 3. JOHN TEBBUTT.

[The quantities computed from Downing's Elements for the epoch 1885·57 are:—Position-angle 201°·65, Distance 14″·07.—ED.]

Red Spot on Jupiter.

GENTLEMEN,—

Jupiter was well visible here in the morning sky as early as Oct. 7; but it was not until today (Oct. 25) that I managed to reobserve the red spot. I turned my 10-inch reflector on the planet at $5^h\ 20^m$ A.M., and immediately saw the red spot advancing near to the central meridian. It was plainer than when I saw it last on July 8, though not so dark as the equatoreal belt, nor nearly so conspicuous as it was five or six years ago. Still it was very distinct at times, notwithstanding the low altitude of Jupiter and somewhat bad definition, and retains much the same size and shape as formerly. I estimated the spot to be exactly central at $5^h\ 32^m$ (=Oct. 24 $17^h\ 32^m$). This agreed very fairly with the time I had computed from my observations last summer, which indicated the transit for Oct. 24 $17^h\ 39^m$. When the planet becomes better situated, I have no doubt the spot will be seen as a prominent object.

The W. side of the depression in the S. border of the great S. equatoreal belt appears now to have disappeared, while the E. side still retains its very definite character. There is now a curious bulging out of dark material from the N. margin of the great northern belt, in nearly the same longitude as the centre of the red spot. These interesting details will be more favourably seen later on.

Yours faithfully,

Bristol, 1885, Oct. 25.

W. F. DENNING.

NOTES.

DR. DUNÉR ON THIRD-TYPE SPECTRA.—An important memoir on stars with banded spectra, which was presented by Dr. Dunér to the Académie des Sciences de Suède on June 11, 1884, has recently been published in a separate form. These beautiful spectra are especially interesting, as the presence of the shaded bands which form their distinguishing characteristic suggests the presence of compound substances in the atmospheres of the stars. It is also noteworthy that they are deficient in violet and ultra-violet light, and that most of the long-period variables belong to this class; and thus in one way or another we are led to infer that they are in an advanced state of development. It would seem therefore more likely that spectra of this kind would afford us examples of change than those of stars manifestly in an earlier stage of their being, and accordingly Dr. Dunér has framed a catalogue of them, with full and copious notes, in order to facilitate the discovery by future observers of any changes in their spectra.

It should be noted that Dr. Dunér's work is not confined to the orange stars, which have spectra conforming to Secchi's third type, but embraces also Secchi's fourth type, the red stars, which likewise have banded spectra, though the bands differ in number, position, and appearance from those in the spectra of the orange stars. Prof. Vogel has preferred to regard these two classes of spectra as but subdivisions of the same type, and calls them therefore type III *a* and III *b* respectively; and Dr. Dunér follows him in this arrangement. Vogel argues that whilst it is easy to imagine a spectrum gradually changing from the first type into the second, and from the second into either of these two forms of banded spectra, and we actually find intermediate forms, it is hardly possible to conceive of a transition-form between the two latter. For whilst the dark bands in the spectra of the orange stars are very dark and sharply defined on the side nearest the violet and shade off gradually towards the red, those in the spectra of the red stars are dark and sharply defined towards the red, and shade off towards the violet. The bands of the orange stars are far more numerous than those of the red, and are so far from corresponding to them in position, that the brightest part of the spectrum in the one class is about the place occupied by one of the darkest bands of the other class. Vogel regards them therefore as denoting not successive stages in a star's development, but as alternative stages.

The catalogue of stars of type III *a* contains 297 objects, that of stars of type III *b*, 55. These lists are themselves important; but the notes which follow are even more so. Each spectrum is described with care and detail; the descriptions of D'Arrest, Pickering, and Secchi are added; the colour of the star is also indicated both in words and by numbers after Schmidt's method. In addition to the name of the stars and their position for 1900·0, their numbers in the B.A.C., the D.M., and the catalogues of Schjellerup and Birmingham are also given. The catalogue concludes with a second notice of half a dozen stars of doubtful type, of which R Andromedæ is the most remarkable. This long-period variable requires the most careful spectroscopic study.

The next section contains a list of 42 stars which Secchi or D'Arrest have believed to be of the third type, but which do not now appear to be so. The only instances, however, where there is much evidence for a change having taken place are 249 Schj., which Dr. Dunér could not find, and which is probably a long-period variable, and D.M. $+36°·2772$, which D'Arrest describes as having a columnar spectrum, but which Dunér found to be a feeble example of the second solar type. His researches lead him to think that marked changes in stellar spectra do not take place quickly.

Great care has been taken in the computation of the wave-lengths of the bands and principal lines in the two classes of spectra. In each class Dunér finds, as previous observers in general have done, that the positions of the sharp edges of the bands are the same for all stars of the same type; the relative extent and intensity of the individual bands, however, vary exceedingly. Dr. Dunér was at a great disadvantage in these determinations, in that he was not provided with the means for procuring any spark-spectra for comparison; but his results seem as accurate as the means at his command would allow—certainly no pains seem to have been spared to render them satisfactory. Several interesting points are brought out by these measures. The first—the remarkable way in which the bands of the orange stars are terminated by strong metallic lines, mostly of iron—has been frequently noted before; the next, the coincidence of the dark bands of the red stars with the bright bands of a carbon compound, so well known as characteristic of the spectra of comets, is exceedingly interesting; and, lastly, Dr. Dunér gives several instances in which he thinks there is strong reason to believe that individual lines in the solar spectrum have changed considerably since the date of Ångström's charts.

In the concluding portion of this memoir Dr. Dunér gives his reasons for believing that stars of type III *b* are 50 times as scarce as those of type III *a*; he also shows that the former are much more numerous in or near the Milky Way than elsewhere, and most numerous in its densest portions. Doberck, Pechüle, and others have previously shown that the same relation holds good for stars of a ruddy tint. A plate, giving six examples of banded spectra, concludes this valuable contribution to stellar spectroscopy.

THE SPECTRUM OF β ORIONIS.—It is curious to find from Dunér's memoir that Prof. Vogel, despite the powerful means at his disposal, his experience, and the difficult spectra he has successfully observed, still fails to recognize the hydrogen lines in the spectra of the Orion stars except Betelgeuse. The lines in the Belt-stars, δ, ϵ, and ζ, are undoubtedly exceedingly difficult; but the F line in β Orionis can be readily seen on any steady night. Not only has the Astronomer Royal both seen and measured it, and his assistants likewise, but Dr. Huggins speaks of it as "stronger than F in the solar spectrum," and Secchi found it quite distinct. Dr. Huggins, further, has found no difficulty in photographing the hydrogen lines in the violet part of the same spectrum. Prof. Vogel also fails to resolve the principal bands of the spectra of type III *a* into fine lines, which both D'Arrest and Huggins have succeeded in doing. It seems clear that spectroscopes and spectroscopists are not exempt from personality.

THE SPECTRUM OF THE GREAT NEBULA IN ANDROMEDA.—An observer at Yale College writes to 'Science' to the effect that he has observed three bright lines in the spectrum of the nebula. If the nebula and not the *Nova* be really intended, this is a most interesting observation. The lines are said to agree as to position with the bright lines in γ Cassiopeiæ and β Lyræ. The first line coincides with Hβ, and the wave-lengths of the other two are given as 5312·5 and 5594·0.

It is frequently argued that the feebleness of the red end of the spectrum of the nebula is evidence of its light having suffered great absorption. It seems to us that the observations of Langley and Vogel rather suggest that the reverse is the case. In the case both of the Sun and of the Earth, it is the blue and green rays which suffer the greatest loss by their passage through the atmosphere, not the red. Could we conceive the Sun's atmosphere wholly removed, and the light of the Sun so enfeebled that its intrinsic brilliancy was no greater than that of the Andromeda nebula, it would give a spectrum not very dissimilar to that of the nebula.

THE NEW STAR IN THE GREAT NEBULA IN ANDROMEDA *.—Dr. Sophus Tromholt relates a curious story respecting this object. In a tale in an Hungarian periodical published in 1851, the author describes both the star and the nebula, and says that the Evil Spirit Asafiel revealed to King Saul and his sons the star in the nebula, and predicted that those who could not see it should perish in the impending battle. The story is said to rest on an old Jewish legend.

MR. E. E. BARNARD, about two years ago, found a moderate-sized nebula in the extreme *p.* end of the great nebula of Andromeda. As he can find no record of this object it is presumed to be new. It is faint, but quite distinct and definite in outline. The place is

$$\left.\begin{array}{l}\alpha=0^{\mathrm{h}}\ 34^{\mathrm{m}}\ 16^{\mathrm{s}}\\ \delta=+40''\ 6'\ 0''\end{array}\right\}\ 1885{\cdot}0.$$

TEMPEL'S COMET OF 1866 AND THE NOVEMBER METEORS.—In a paper on this subject read before the American Philosophical Society on July 17, 1885, Prof. D. Kirkwood points out that there are three distinct meteoric clusters moving in the orbit of Tempel's Comet. The first is the well-known meteoric group, the motion of which has been discussed by Adams, Newton, and Schiaparelli, the periodic time having been found to be 33·25 years. In 1875 Prof. Kirkwood called attention to the fact that the dates of several meteoric showers, given by Humboldt and Quetelet as belonging to the November stream, indicated the existence of a second cluster, moving in very nearly the same orbit. The periodic time of this group appears to be 33·31 years. The third cluster has been less observed, and is probably less extensive as well as less dense. From observations extending from 1582 to 1880, Prof. Kirkwood concludes that the periodic time is 33·11 years. It will be remembered that the periodic time of Tempel's Comet is, from Oppolzer's elements, 33·176 years, although later researches give 33·28 years as the more probable period. The next return of these several bodies may be expected as follows:—Tempel's Comet in 1899, Group I. in 1899 to 1901, Group II. in 1886 to 1889, and Group III. in 1912 to 1915. With regard to the probable epoch of the separation of Group I. from the original mass, Prof. Kirkwood points out that the display of 1366 was contemporaneous, or nearly so, with the appearance of the comet, and that the latter has undoubtedly greatly diminished in magnitude during the last 500 years, suggesting the idea that the less apparent magnitude may be a consequence of separation at that epoch.

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* 'Nature' for Oct. 15, 1885, p. 579.

THE RIGHT ASCENSION OF CERTAIN FUNDAMENTAL STARS [*].—In the course of his investigations with the Almucantar ('Observatory,' No. 78, p. 305), Mr. S. C. Chandler, jun., has found that, in the case of certain stars not far from the Pole, the right ascensions of the 'Berliner Jahrbuch' seem to require sensible corrections, if the indications of the instrument are to be trusted. For three of these stars, namely 1 (H) Draconis, η Draconis, and Groomb. 2377, the existence of deviation from Auwers' places has already been shown by Prof. Rogers, and the amount of Mr. Chandler's corrections agrees very closely with the values Rogers has given. With regard to the other five stars, viz. ν Ursæ Majoris, γ Ursæ Minoris, 19 Ursæ Minoris, 48 (H) Cephei, and η Ursæ Minoris, Mr. Chandler finds that the corrections to the 'Berliner Jahrbuch,' deduced from recent Greenwich and Cambridge (U.S.) observations, agree generally with the Almucantar values, and hence concludes that in most of these cases the adopted proper motion requires modification in order to satisfy the more modern observations. It must be remarked, however, that the majority of the stars whose places are here discussed are in the neighbourhood of the Pole, and the determination of systematic corrections in R.A. at such large declinations is a matter of extreme difficulty, so that the detection of errors in adopted proper motions from observations extending over a few years only must, in such cases, be attended with considerable uncertainty. The general result of the comparisons made by Mr. Chandler is, however, favourable to the trustworthiness of the Almucantar observations.

THE "ANONYMOUS" STARS OF YARNALL'S CATALOGUE [†].—Prof. E. Millosevich has published a useful contribution to exact astronomy in his paper "Sulle Stelle boreali fino a −1° inclusivo dette *Anonymous* del Catalogo di Yarnall (Washington, 1878)." It appears that, between the limits +90° and −1° declination, Yarnall's Catalogue contains 732 stars which are designated as anonymous. Prof. Millosevich has carefully gone through these, comparing them with the DM. or with the Catalogues of Weisse, Rümker, Struve, Lalande, or Oeltzen's Argelander, and has succeeded in almost every case (sometimes by making conjectural corrections to Yarnall's places) in identifying the star in the Washington Catalogue. The paper also contains a comparison of the estimates of magnitude made by the Washington observers and by Argelander in the case of these anonymous stars. After applying a systematic correction of +0·3 of a magnitude to Yarnall, Prof. Millosevich finds that there are 33 stars which differ by 0·9 of a magnitude or more in the two catalogues, and gives a list of these with their places for 1860·0, as there is a strong presumption that they will prove to be variables. Taken in conjunction with Holden's "List of Errata in Yarnall" (Astron. Nachrichten, No. 2561), Prof. Millosevich's paper is a useful addendum to the Washington Catalogue for 1860.

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[*] Astron. Nachrichten, No. 2687.

[†] 'Annali della Meteorologia Italiana,' Part iii. (1884).

PHOTOGRAPHING THE CORONA.—Mr. W. H. Pickering has made some further experiments with regard to photographing the corona in full sunshine, and has given an account of his results in a lengthy communication to 'Science' for Aug. 14, 1885. We are unable to reproduce this paper in full, but the following are the principal points:—

Various observations made during total eclipses have shown the corona to be, on the average, of about the same brightness as the full Moon. But it is not sufficient for a photograph simply to show the corona as a uniform ring of light round the Sun, it must show something of the rays and rifts; and this implies that it must be able to show contrasts of light which do not vary from one another by more than one tenth the intrinsic brilliancy of the full Moon. He considers that photographs are less able than the eye to distinguish small differences of light, and states that it often is impossible to photograph a distant mountain which is yet easily visible, and that the Moon cannot be photographed in the daytime when the Sun is high above the horizon, though it may be perfectly distinct to the eye.

Beside these general considerations, Mr. Pickering gives the results of several investigations he has been carrying on in order to throw further light on the subject. He states that his plates will not show differences of light smaller than five per cent., or for images so small as the corona would be, than ten per cent. Since the corona at 3′ from the Sun reflects 0·1 as much light as the full Moon, to distinguish between a rift and a ray at that distance from the Sun the atmospheric illumination ought not to exceed the full Moon in intrinsic brightness. Mr. Pickering finds, however, that it is at least 300 times as bright, even in the clearest weather. We print elsewhere Dr. Huggins's reply to these various arguments.

Mr. Pickering has altered his apparatus, substituting a spectacle-glass for his achromatic lens, and has obtained a vast improvement in the results. A photograph on a clear day gave not the slightest trace of a fringe; yet in his communication to 'Science,' May 29, 1885—in which he criticized in strong terms Dr. Huggins's description of the results he obtains from a cloudless but whity sky, viz. that the Sun was "well defined upon a sensibly uniform surrounding of air-glare, but without any indication of the corona"—he had said that it was evident that a great part of the corona-like fringe shown in his own (Mr. Pickering's) photographs was due to causes outside of the instrument, and hence could not be diminished by changes in the latter. But although he has thus got rid of the glare which before he declared to be inseparable from these photographs, he has not yet succeeded in obtaining any true image of the corona, and he considers that his want of success is in accordance with the result which his theory has indicated.

THE LUNAR PHYSICAL LIBRATION *.—M. Ch. Simon has published, in Tome iii. of 'Annales de l'Ecole Normale,' an interesting memoir on the theory of the Moon's libration. He shows that if we neglect the eccentricity of the lunar orbit, it can be proved that the axis of rotation of the Moon is displaced (in the interior of our satellite), so as to oscillate continuously in the plane perpendicular to the principal axis which is directed towards the Earth. Under the same conditions, Poisson had found that the axis of rotation describes a cone of revolution. M. Tisserand's attention having been thus directed to the subject, he finds that M. Simon is quite accurate in his conclusions; and that the discordance between his result and that arrived at by Poisson is removed when two of the coefficients employed by the latter in his formulæ are calculated more accurately. This M. Tisserand proceeds to do, and thus demonstrates that M. Poisson's process is correct, and leads to a correct result when the more accurate values of the coefficients referred to are inserted in the formulæ.

THE WEDGE-PHOTOMETER.—In No. 2680 of the 'Astronomische Nachrichten' is a lengthy article by Dr. Wilsing on this subject. In his opinion the principle of the wedge-photometer has, up to the present, received insufficient treatment. The wish to employ it in observations of variable stars, and also to possess a more ready means of determining relative brightness than by the old method of grades, induced him to undertake a series of experiments whereby to judge of its fitness. In respect to the homogeneity, selective absorption, and geometrical figure of the wedge, he considers that Prof. Pritchard has exhausted the subject; but it is quite different in respect to the physiological side of the question. The sensibility of the eye enters as an important factor in estimations made with the wedge-photometer. Fechner and Pickering have both shown the importance of the intensity of the background in this connection. In the 'Astron. Register,' 1882 Jan., the latter says:—"The use of a graduated wedge of glass to reduce the light .... does not remove the objection that the field

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* 'Comptes Rendus,' Tome ci. No. 13.

is darkened with the star, so that a bright star at its limit of visibility is seen on a comparatively dark background." To bear out this, Dr. Wilsing exhibits a table of results of his own experiments, which shows influences of a large order. Later on in his paper he points out that the influence of the intensity of the background begins to show (with Prof. Pritchard's telescopes of 4 and 3 inches aperture) with stars fainter than the 4th mag. Below is a comparison between stars observed by Prof. Pritchard and Prof. Pickering. Column 1 shows the included magnitudes, column 2 the difference Pritchard − Pickering, and column 3 the number of comparison-stars.

1.	2.	3.	1.	2.	3.
— up to 1·0	+0·17 mag.	6	3·0 up to 4·0	+0·03	138
1·0 „ 2·0	+0·06 „	14	4·0 „ 5·0	+0·15	246
2·0 „ 3·0	0·00 „	45	5·0 upwards	+0·44	37

Much stress also is laid on the inconstancy of the sensibility of the eye. The amount of fluctuation far exceeds the accuracy claimed for the wedge-photometer. The results of two nights' observations of two stars used for comparison with the variable U Cephei give a range of scale-readings amounting to as much as 0·3 mag. and 0·6 mag. respectively. Another, and very important, point has received Dr. Wilsing's attention. He deduces the value* of the "Wedge Interval" in three distinct ways, and obtains the magnitude corresponding to one millimetre, as follows :—

	Wedge I.	Wedge II.
The method of telescopic apertures ..	0·137 mag.	0·236 mag.
Polarization	0·110 „	0·210 „
Comparison of his own observations with Pickering's	0·087 „	0·153 „

He therefore believes that we are not entitled to treat the value of the reduction factor, as deduced by the telescopic method, in the sense of an instrumental constant.

With respect to coloured stars, Dr. Wilsing remarks that—"If the limit of sensibility, independent of colour, is estimated only by the amplitude of vibration, as Prof. Pritchard has done, the wedge-photometer will furnish a relative intensity not accordant with the direct estimation ;" and asks—"Does the wedge-photometer give equal intensities for different coloured, though physiologically equally bright, stars ?" An interesting table, showing the relative sensibility of the eye for different colours of the spectrum, exhibits a range of 2·5 mag. in estimations made in the red and blue respectively. In conclusion, Dr. Wilsing thinks :—(1) That the use of the method of limiting apertures for the determination of the wedge constant is to be avoided, because the brightness in individual objectives is not directly proportional to the aperture. (2) That it is not right to assume the invariability of the eye for any length of time. (3) That disturbing influences make themselves felt, particularly in the fainter stars, where the brightness of the ground forms an important fractional part of the total intensity. (4) That comparisons of different coloured stars are in general not comparable immediately with direct estimations. With these reservations, he looks upon the wedge as a valuable increase to our aids.

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* It will be remembered that Prof. Pritchard's results were, for the two methods, 1·945 and 1·86 mag. to an inch, or 0·0766 and 0·0732 mag. to a millimetre.

SMALL *v.* LARGE TELESCOPES.—Prof. E. S. Holden writes to the 'Sidereal Messenger' for October to say that he entirely agrees with Mr. Burnham's remarks on this question, published in the 'Sidereal Messenger' for September ('Observatory,' No. 102, p. 342).

CONJUNCTIONS OF SATURN WITH SMALL STARS.—Mr. Marth calls attention, in the 'Monthly Notices' for June, to the following conjunctions:—

Nov. 18 $2^{\mathrm{h}}\cdot4$ or $4^{\mathrm{h}}\cdot3$ ☌ of DM. $22^{\circ}\cdot1424$ with Saturn, $+62''$ *s.*
Dec. 9 4 ·4 ☌ of DM. $22^{\circ}\cdot1383$ with *p.* edge of ring, $y = -\ 8''\cdot0$.
6 ·4 ☌ with centre of Saturn, $y = -12''\cdot2$.
8 ·5 ☌ with *f.* edge of ring, $y = -16''\cdot4$.

The magnitudes of the stars are about 8·8. The conjunction on Dec. 9 may prove to be an occultation, and should be therefore specially watched.

OBSERVATIONS OF DOUBLE STARS.—Dr. R. Engelmann has recently published an important paper * containing the results of his researches in this interesting branch of sidereal astronomy. The observations were made with a Clark-Repsold refractor of 7½-inch aperture and filar micrometer; and every care appears to have been taken to secure accuracy in the results and to render the series of measures worthy of the reputation of the author. Altogether the paper contains more than 2500 measures of 389 pairs, and each object has been measured in position-angle (and generally in distance) on at least five different evenings. The stars observed have been selected from the catalogues of F. G. W. and of O. Struve. A few of Burnham's pairs have also been measured. The observations extend over the period 1882–84. Dr. Engelmann has discussed the various sources of systematic and constant errors to which double-star measures are liable, and has corrected the observed distances for discordances arising from the magnifying-power employed, from the use of illuminated wires in a dark field (the illuminated field being taken as the standard), and from a different method of measurement which has been used in the case of the closest pairs. The distances measured in the early part of 1882 have also been corrected by a constant quantity, by which they differ from subsequent observations. In all these cases Dr. Engelmann has found that the deduced corrections to the position-angles are not sufficiently certain to justify him in applying them. With regard to systematic error arising from inclination of the line joining the components of the double-star to the vertical, the author has observed, at different hour-angles, many of the stars which have been selected by O. Struve as tests for this purpose, and finds that the discordances are not much greater than the mean errors of the results. A comparison is also made between the observations of these test-objects by Engelmann, Dembowski, and Hall, which is eminently satisfactory. The paper is a valuable addition to the literature of double stars.

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* Reprinted from Astron. Nachrichten, Nos. 262–63 & 2677–78.

Mons. Perrotin has also published* a series of measures of double stars (in continuation of a former series) made at Nice with the Gautier Equatoreal of 0m·38 aperture. The stars have been selected from the catalogues of the Struves, and M. Perrotin states that many of the pairs which have been set down as difficult, in the different lists of double stars, are easily measured at Nice. Each object has been observed, in general, on two or three separate evenings. The observations were made in 1883 and 1884.

We append the Leipzig and Nice mean results for a few of the more interesting stars :—

Date.	Object.	Pos.	Dist.	Observer.	Date.	Object.	Pos.	Dist.	Obse ver.
		°	″				°	″	
1882·87	η Cassiopeiæ.	165·70	5·155	E.	1883·07	γ Virginis.	155·59	5·222	E.
2·88	Castor (A, B).	234·29	5·559	”	4·38	”	155·73	5·427	P.
4·17	” ”	232·15	5·840	P.	2·93	42 Comæ.	192·11	0·560	E.
2·22	ζ Cancri (A, B).	76·18	1·051	E.	2·61	η Coronæ.	153·17	0·558	”
3·24	” ”	72·45	1·047	”	3·51	”	153·16	0·510	”
4·28	” ”	67·02	0·938	”	4·64	”	165·64	0·578	”
4·17	” ”	62·85	0·985	P.	4·52	”	163·15	0·642	P.
2·23	ω Leonis.	80·01	0·559	E.	2·76	ζ Herculis.	106·97	1·754	E.
3·24	”	85·83	0·620	”	3·72	”	102·52	1·650	”
4·23	”	91·42	0·658	”	4·71	”	98·77	1·890	”
4·18	”	90·65	0·555	P.	4·55	”	94·13	1·470	P.
2·24	γ Leonis.	114·33	3·521	E.	2·72	p Ophiuchi.	51·62	2·314	E.
3·27	”	112·66	3·344	”	3·72	”	43·62	2·251	”
4·19	”	113·30	3·463	P.	4·69	”	35·26	2·204	”
2·25	ξ Ursæ Maj.	262·10	1·987	E.	4·56	”	34·52	2·088	P.
3·32	” ”	257·83	2·002	”	2·84	δ Cygni.	321·44	1·752	E.
4·41	” ”	249·60	1·920	”	3·82	”	321·47	1·791	”
4·28	” ”	248·02	1·687	P.	3·25	β Delphini.	183·90	0·194	”
3·07	ι Leonis.	84·57	2·760	E.	4·71	”	197·75	0·320	”
1884·20	”	63·95	2·577	P.	2·54	Sirius.	43·98	……	”
					1884·05	”	36·03	9·67	P.

In the column “Observer,” E = Engelmann, P = Perrotin.

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* Astron. Nachrichten, Nos. 2684, 2685.

ASTRONOMY AT THE AMERICAN ASSOCIATION.—Prof. Newton read a paper on "The effect of small bodies passing near a planet on the planet's velocity," in which he shows that a cylindrical stream of small bodies, evenly distributed, and all moving in the same direction with a common velocity past the Earth supposed to be in the axis of the cylinder, will communicate to the Earth in each unit of time a velocity along the axis—1, proportional to the density of the group; 2, that decreases *nearly* inversely as the square of the group's velocity; 3, that increases as the logarithm of the radius of the cylinder, the radius being measured by a unit differing little from the Earth's radius. He then proceeds to show the effect of a widely extended group of small bodies equally distributed in space, with equal velocities, but directed towards points evenly distributed over the celestial sphere, with the Earth moving in a right line through them, and, lastly, of bodies striking the earth. We must refer our readers to the paper itself for further details. Coming, as it does, after Mr. Denning's announcement of the fixity of long-continued meteor-radiants, this paper is of much interest and importance. Prof. Harkness read a paper on the "Flexure of the Transit instrument," in which he advocated the use of a zenith collimator. Prof. Hough read a paper describing recent improvements in the printing chronograph. In the physical Section, the first paper read was by Prof. Langley, on the spectra of some sources of invisible radiations, and on the recognition of hitherto unmeasured wave-lengths. It has been known from the time of the first Herschel that heat radiations existed below the range of vision, but it was supposed that glass absorbed this dark heat. In 1881 Langley found that common glass was diathermanous to all the dark rays received from the Sun, and determined the wave-lengths of a newly discovered solar region by direct observation. We have in this infra-red portion of the Sun spectrum the greater part of the heat which sustains organic matter on this planet; and the questions arise, Does the planet radiate heat of the wave-lengths that it receives from the Sun? and how is its temperature maintained, probably several hundred degrees above the temperature of space, when our observations show that the direct radiations of heat from the Sun can only raise it about 50° above the surrounding temperature? Experiments at Alleghany show that the dark solar heat is transmitted by our atmosphere with less difficulty than the light; and if the radiations of the soil are of this wave-length, our planet should actually be cooler on account of its atmosphere than if it had none. To clear up this point has been the aim of Prof. Langley's work. He used a prism and lenses of rock-salt, and for radiating surfaces Leslie cubes covered with lampblack and filled with boiling water. Nearly the whole heat-spectrum from these sources passed through the prism at angles which the theories of our text-books have heretofore pronounced impossible. Speaking with reserve, Prof. Langley says that we have every reason to believe that heat radiated by the soil has a wave-length twenty times that of the lowest visible line of the solar spectrum. Prof. Langley's paper was followed by one by Mr. J. A. Brashear on a practical method of working rock-salt surfaces for optical purposes.

UNIVERSAL OR COSMIC TIME *.—The present powerful movement in favour of a universal Prime-Meridian and Time System owes its origin principally to the exertions of Mr. Sandford Fleming, who read two papers on these subjects before the Canadian Institute in 1879. The Institute took up the matter warmly, and succeeded in bringing it under the notice of the various Governments, leading eventually to the Washington Conference; and the present pamphlet contains a brief but clear history of the movement with, as appendices, the principal documents which it called forth. This is not the place to discuss the advantages or disadvantages of the proposed change; it is sufficient to say that the pamphlet is valuable as the record of so important a movement, and great credit is due to Mr. Fleming for the energetic way in which he brought his views before the public. We may, however, repeat what we have said before, that, so far as the question of the adoption of the new time by astronomers is concerned, the decision really lies in the hands of the chiefs of the four great Nautical Almanac Offices. Until these adopt it, and until they *all* adopt it—for it would be a misfortune if the change were made in one great Ephemeris but not in the others—astronomers must perforce adhere to the old reckoning in all their publications if a most serious confusion is not to be introduced.

THE LICK OBSERVATORY.—From time to time there has been noted in these pages the progress which has been made in the buildings and establishment of the instruments of the Lick Observatory, and, quite recently, in an article by Prof. Holden on the subject ('Observatory,' No. 95, pp. 84–86) will be found an interesting account of much that has been done up to the present. In a recent number of 'Science' (Vol. vi. No. 135) there is a considerable amount of information respecting this splendidly endowed and splendidly equipped institution. Prof. D. P. Todd writes on the future of the Lick Observatory, pointing out the advantages which the astronomers who may be privileged to work in it will enjoy, on account of its great elevation, as well as on account of the great excellence and power of the instruments which will be at their disposal. With regard to the former, Prof. Todd points out that the gain in steadiness of the atmosphere at this elevated station has already been proved, by experiments made by Mr.

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* 'Universal or Cosmic Time.' By Sandford Fleming, C.E., C.M.G., &c., together with other papers, communications, and reports in the possession of the Canadian Institute, respecting the movement for reforming the Time System of the World, and establishing a Prime Meridian as a zero common to all nations. Toronto: Copp Clarke and Co.

Burnham and by himself, to be much greater than any one expected at the outset; and at the same time the elevation, above a mile of the lower atmosphere, makes effectively available a much larger region of sky than can be commanded at other stations in a like latitude. The instrumental equipment of the Observatory (an account of which is given in Prof. Holden's article referred to above), although incomplete, is, as Prof. Todd says, an unusual one, and, in its final state, will surpass that of all other Observatories. We understand that the crown disk for the 36-inch refractor has now been delivered to Alvan Clark and Son, by MM. Feil and Mantois, the flint-glass having been prepared and sent over by the same firm some three years ago, so that there appears to be no reason why the great telescope should not be completed in 1887. There is a second article, in the number of 'Science' before referred to, on the same subject. It contains a description of the buildings and grounds of the Observatory, with illustrations, and gives the reader a very clear idea of the difficulties which presented themselves to the trustees, and of the skill with which they have been overcome. With regard to the prospective capabilities of the great telescope when placed in so favourable an atmosphere, it is stated that it is not unreasonable to suppose that on the best nights the maximum magnifying-power (about 3500) may be advantageously employed. Making due allowance for the unavoidable effects of the Earth's atmosphere, the observer might, under these circumstances, expect to see the Moon much the same as he would without the telescope if it were only 100 miles away. It might be possible then to make out details of objects even although they were no larger than some of the larger edifices on the Earth. However this may be, it must be conceded that no Observatory is so well fitted to carry out investigations requiring large aperture and good atmospheric conditions as that situated on the summit of Mount Hamilton. Of course a necessary element in the success of the institution is that the *personnel* should be carefully and judiciously selected; and astronomers in this country will look with interest for intelligence respecting appointments to the staff of the Lick Observatory.

LIGHT AS A MEANS OF INVESTIGATION *.—These four lectures form the second course of Prof. Stokes's Burnett lectures. The first series treated of the nature of light, the present series deals with the discoveries we have been able to make by means of the properties of light in their relation to ponderable matter. The principal portion of the book is therefore concerned with the application of the spectroscope to astronomy, and so comes within our special province. This subject occupies more than half the second chapter, and the whole of the third and fourth, and in this space we have a wonderful amount of information presented to us, not by any means after the methods of ordinary text-books, for principles are more dwelt upon than mere lists of facts, though nothing of importance is omitted, and these are most carefully enunciated and explained. It is needless to say how valuable to the student these explanations by a master so accurate and able as Prof. Stokes must be, for we find here the very points made plain which popular writers are apt to slur over or misrepresent. Amongst the more interesting points may be instanced the history of the discoveries which have made spectroscopy a branch of astronomy, including the explanation of Prevost's theory of exchanges, a history which is only marred by Prof. Stokes's silence as to the part he himself took in the work. The section on comets with Prof. Stokes's theory as to their constitution and the explanation of the spectroscopic method of determining motions in the line of sight deserve particular attention. The concluding pages are of special importance, dealing as they do with the constitution of the Sun, and summarizing and digesting the information we really possess about it, and leading up naturally and easily to the tasteful little exordium which concludes this interesting and valuable little book.

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* Burnett Lectures on Light. Second Course. "On Light as a Means of Investigation." Delivered at Aberdeen in December 1884. By George Gabriel Stokes, M.A., F.R.S. London: MacMillan and Co.

TELEGRAPHIC DETERMINATIONS OF THE DIFFERENCE OF LONGITUDE BETWEEN KARACHI, AVANASHI, ROORKEE, PONDICHERRY, COLOMBO, JAFFNA, MUDDAPUR AND SINGAPORE, AND THE GOVERNMENT OBSERVATORY, MADRAS. By NORMAN R. POGSON, Government Astronomer at Madras.—We have here the results of operations undertaken by Mr. Pogson, in concert with other astronomers, during the years 1870 to 1875, for determination of the longitudes of the places named, in most cases in consequence of requests made to him by observers of the total solar eclipse of 1871 and of the transit of Venus in 1874, desirous of fixing the positions of their observing-stations; and the results presented evidently represent a good deal of troublesome work. The differences of longitude between Madras and the various places mentioned were all, as is indicated, determined telegraphically, the fundamental longitude of Madras, taken as $5^h\ 20^m\ 59^s{\cdot}4$, being "a combination result, in which the telegraphic longitude of Suez, by members of the Transit of Venus Expedition in 1874, was, combined with others, between Suez, Aden, Bombay, and Madras, made by the officers of the Great Trigonometrical Survey of India in 1876 and 1877." The local time at the various stations was determined in general by meridian-transits, the exceptions being that at Colombo and Jaffna it was found from double altitudes of the Sun, using a three-vernier reflecting-circle by Troughton and Simms and mercurial artificial horizon, whilst at Muddapur it is not known in what way time was determined. Exchanges of telegraphic signals were made on one night only with Karachi, Avanashi, Pondicherry, and Muddapur, but on two or more nights with all the other stations. Some difficulties could not apparently be overcome. Thus, although the time-determinations at Madras were reduced by application of the known personal equations to the method of observing of Mr. Pogson as standard, no opportunity occurred of making comparison with the methods of the observers at the other stations, excepting in the case of Avanashi, so that as regards the remaining places, as Mr. Pogson says, "the small difference due to personal equation between myself and the operator at the other end remains in each case indeterminate." It appears however by the concluding remarks that, in instances in which comparison with other determinations was possible, there is general agreement of results. Mr. Pogson seems to have wisely taken advantage of such opportunities as accidentally arose to carry out, as completely as circumstances would permit, operations leading to useful results which otherwise might not have been obtained. W. E.

THE PHAINOMENA OF ARATOS *.—Mr. Robert Brown, the author of 'The Great Dionysiak Myth,' has here given us a translation in blank verse of the well-known poem of Aratos. It is scarcely a work which affords much scope for the exercise of high literary gifts; for it must be confessed that the greater portion of the original is rather dull and monotonous reading, and Mr. Brown has unnecessarily restricted himself by attempting to render it exactly line for line. The result is not always pleasing to the ear or clear to the mind; still the translation appears to be a good and accurate one, and compares not unfavourably in these respects with Dr. Lamb's; and in the more ornate passages of Aratos, as the opening lines and the legend of Astræa, the lines flow clearly and easily enough. But, as those who know Mr. Brown's other works will readily believe, the actual translation is the least part of the book; the most important portion consists of the abundant and valuable notes upon every constellation named. Mr. Brown's erudition and range of research have received recognition ere now from the highest authorities, and they are displayed afresh in these interesting notes. Many of the constellations are traced back to their Akkadian stage, as far perhaps as we shall ever be able to go, though astronomy teaches us beyond a doubt that for their first designing we must look to a more northern land and an earlier epoch still. The suggested identification of the legendary Babylonian kings with the principal stars of the zodiac is very ingenious and not improbable; and Mr. Brown's remarks as to the date of the observations upon which the poem of Aratos is founded are very forcible. The book is adorned with a great number of curious diagrams, mostly Middle Age or Archaic designs of the constellation figures; and concludes with the striking and vigorous Song of Athénaié.

* 'The Phainomena, or "Heavenly Display" of Aratos: done into English Verse.' By Robert Brown, jun., F.S.A., author of 'The Great Dionysiak Myth' &c. London: Longmans, Green, and Co. 1885.

THE FORCE OF GRAVITY.—The Appendix to the Memoir No. 5 of Tokio University contains some interesting results. The observed force of gravity in the neighbourhood of Japan has always been somewhat in excess of the values derived from the ordinary formulæ. In the present instance, pendulum experiments were conducted at Tokio and Ogasawarajima (Bonin), the resulting value of "g" for the latter place being in C.G.S. units 979·472. The values deduced from the formulæ of Everett, Listing, and Herschel are respectively 979·139, 979·126, 979·135, all of which fall short of the observed value by about ·034 p. c. At the same time the magnetic constant H and the declination were observed. For Tokio H=·2960 and for Ogasawarajima H=·3166, the mean declination at the latter being 2° 3′ 8″ W.

RESAL'S 'MÉCANIQUE CÉLESTE'[*].—The first edition of this work was published in 1865. The author has taken the opportunity afforded him by a call for a second edition of considerably enlarging the book, so that it may more completely fulfil the conditions of explaining the fundamental principles of the subject in a manner sufficiently simple to meet the requirements of students of Astronomy in the French Colleges.

As preliminary to the main portion of the work, M. Resal has treated in the Introduction certain questions of analytical mechanics and of analysis, a knowledge of which is required for an intelligent perusal of the book; such as:—the general equations of motion of a system of material points due to Lagrange, Hamilton, and Jacobi; the employment of elliptic co-ordinates in the solution of certain problems relating to the motion of a material point &c. The Author then proceeds to discuss elliptic and parabolic motion, gravitation, and the determination of the masses of heavenly bodies. He gives Gauss's method of determining the constants introduced into the formulæ of elliptic motion—a method which is not mentioned either by Laplace or by Pontécoulant. The next chapter is devoted to the theory of perturbations. M. Resal has most wisely, before discussing the problem analytically, introduced the reader to the geometrical method of considering perturbations, the first idea of which is due to Newton, and which was afterwards developed by Lagrange. The following chapters are on the theory of attraction and the figure of heavenly bodies and of their atmospheres; of the motion of these bodies round their centre of gravity; of tides—oceanic and atmospheric; of terrestrial heat, with an investigation of the effect of the secular cooling of the Earth on the duration of the mean day; of astronomical refraction; of inequalities in the motion of planets due to the ellipticity of the Sun, with application to the case of Mercury; and an introduction to the theory of the Moon, following Laplace's method.

The book appears to be admirably adapted for educational purposes, without going into those tedious details which are necessary for the requirements of the practical Astronomer.

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[*] Traité Elémentaire de Mécanique Céleste, par M. H. Resal, Membre de l'Institut, Professeur à l'Ecole Polytechnique. Deuxième Edition. Paris: Gauthier-Villars, 1884.

Star with large Proper Motion*.—Herr Berberich has pointed out that the star Weisse (1) X 1021 appears to have a large proper motion in right ascension, as is shown by comparing the following three positions for 1885·0:—

Authority.	Epoch.	R.A.	Dec.
Weisse	1824	$10^h\ 58^m\ 17^s.83$	$+8^\circ\ 47'\ 10''.3$
Lamont	1845	10 58 17·17	+8 47 9·7
Berlin Merid. Obs. .	1885	10 58 16·37	+8 47 9·6

Identity of Denning's and Biela's Comets.—A writer in the 'American Journal of Science' remarks upon this subject that, "if the radiants for the two comets be compared, they will be found to differ in position by a distance of 125° or 130°, and this distance is a measure of the necessary disturbing power of the Earth in order to throw the comet from one orbit into the other. Either comet, it will be found by a simple computation, would have to come much nearer to the Earth's centre than 4000 miles in order to suffer such a perturbation of orbit." The curious correspondences pointed out by Mr. Denning have not, therefore, the significance he was inclined to attach to them.

New Minor Planets.—A new minor planet, No. 251, mag. 13·5, was discovered on Oct. 4 by Herr Palisa at Vienna, whilst searching for Eudora, No. 217. Another, No. 252, was discovered on Oct. 27 by M. Perrotin at Nice.

Dr. Gill, writing to the Editor of the 'Astronomische Nachrichten,' begs his advocacy in the following matter. Dr. Gill thinks that among the numerous European and American observatories one might be found, the energies of which might be devoted to the determination of places of stars observed with comets, faint stars of which occultations have been observed, zones of stars employed for scale or screw values, or stars that have been used for geodetic or other purposes. Dr. Krueger thinks the only difficulty would be in regard to the southern stars, and states that Prof. Hirsch, of Neuchatel Observatory, intends taking up this branch of observation. He hopes later to be able to publish others that are willing to cooperate.

In No. 2688 of the 'Astronomische Nachrichten,' Prof. Safarik gives results of some of his observations of variable stars. There are six maxima of U Geminorum which are set down to 1883, Oct. 24·5; 1884, Jan. 21; 1884, May 10·4; 1884, Oct. 21; 1884, Dec. 24·5; 1885, April 6·5. He remarks that the period Oct. 21 to Dec. 24·5, =64·5 days, is the shortest known to him. Prof. Schönfeld observed a period of 70 days from 1870 Jan. 19 to March 30.

* Circular zum Berliner Astron. Jahrbuch, No. 260.

THE red star Schjell. 238 (AOe$_2$ 20363) (R.A. = 20^h 8^m 19^s, and Decl. $-21°$ 46′ 30″, 1850) Prof. Šafarik finds to be a variable.

THE red star No. 59 in Prof. Pickering's list, published in the Astr. Nach. 2405, has also received Prof. Šafarik's attention since 1883, May. From his observations, which have been taken regularly since that time, he deduces:—

Principal Max. 1883, Aug. 1, and 1884, June 9.
„ 1883, Nov. 26, „ 1884, Dec. 1.

In the Proc. Amer. Acad. xx. this star is 91 *b* in Prof. Pickering's list. He gives there a max. 8·6 and min. 10·5 mag., and a period of 180 days.

DR. DE BALL, the Observatory, Ougrée, near Lüttich, wishes to compute the orbit of Comet 1881 III., and asks for unpublished observations of the same.

Differences of Right Ascension and Declination between Iapetus and the Centre of Saturn. By A. MARTH.

Iapetus—Saturn.

Greenwich Noon.	α—A.	δ—D.	Greenwich Noon.	α—A.	δ—D.
	s	″		s	″
Dec. 1	+24·8	− 49	Dec. 17	+43·0	+140
2	27·6	37	18	42·0	147
3	30·2	24	19	40·7	154
4	32·7	− 11	20	39·2	160
5	34·9	+ 2	21	37·4	165
6	36·9	15	22	35·4	169
7	38·7	28	23	33·2	172
8	40·3	41	24	30·8	174
9	41·7	53	25	28·1	175
10	42·7	66	26	25·3	175
11	43·6	78	27	22·4	173
12	44·1	90	28	19·3	171
13	44·5	101	29	16·0	167
14	44·5	112	30	12·7	163
15	44·3	122	31	+ 9·3	+157
16	+43·8	+131			

Satellite of Neptune.

Greenwich Mean Times of the North-east Elongations.

Dec. 2....	6^h·9	Dec. 14....	1^h·1	Dec. 25....	19^h·3
8....	4 ·0	19....	22 ·2	31....	16 ·4

Position-angle of minor axis of apparent orbit on Dec. 1 = 320° 50′; on Dec. 31 = 320° 19′.

Semi-major axis of orbit on Dec. 1 = 16″·95; on Dec. 31 = 16″·79.

A diagram showing the apparent orbit is given on p. 318.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can usually be only seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS (East).

	h
Dec. 1	14·8
2	13·4
3	12·0
4	10·6
5	9·2
6	7·9
7	6·5
15	18·0
16	16·6
17	15·2
18	13·8

MIMAS (West).

Dec. 7	18·0
8	16·5
9	15·1
10	13·7
11	12·3
12	10·9
13	9·5
14	8·1
15	6·7
25	15·5
26	14·1
27	12·7
28	11·3
29	10·0
30	8·6
31	7·2

ENCELADUS.

	h
Dec. 1	15·4
3	0·2
4	9·1
5	18·0
7	2·9
8	11·8
9	20·6
11	5·5
12	14·4
13	23·3
15	8·1
16	16·9
18	1·8
19	10·7
20	19·6
22	4·5
23	13·3
24	22·2
26	7·1
27	16·0
29	0·9
30	9·7
31	18·6

TETHYS.

	h
Dec. 2	16·4
4	13·7
6	11·0
8	8·3
10	5·6
12	2·9
14	0·2
15	21·4
17	18·7
19	16·0
21	13·3
23	10·5
25	7·8
27	5·1
29	2·4
30	23·7

TITAN.

Dec. 5	16·3
21	13·6

HYPERION.

Dec. 18	3·5

DIONE.

	h
Dec. 1	8·8
4	2·5
6	20·1
9	13·8
12	7·4
15	1·1
17	18·7
20	12·4
23	6·1
25	23·8
28	17·3
31	11·0

RHEA.

Dec. 4	5·5
8	17·7
13	6·0
17	18·3
22	6·5
26	18·8
31	7·2

Apparent Elements of the Ring.

Greenwich Noon.	Position-angle of Minor Axis.	Latitude above plane of Ring. Earth.	Latitude above plane of Ring. Sun.
1885, Dec. 4	.... 353° 14′	25° 42′ S.	26° 11′ S.
19	.... 353 18	25 54 S.	26 7 S.

Greenwich Noon.	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Semi-maj. axis of Orb. of Titan.
1885, Dec. 4	.. 46″·21	20″·03	30″·73	13″·32	207″·9
19	.. 46 ·60	20 ·35	30 ·99	13 ·53	209 ·6

The semi-major axis of the orbit of Titan is given as well as the position-angle of the minor axis of the ring, in order to furnish means for roughly inferring the distances and position-angles of the satellites from the diagram given on p. 316.

Maxima and Minima of Variable Stars in December 1885.

M, signifies maximum ; *m*, minimum.

Dec.	5	S Tauri, M.
	5·3	W Sagittarii, M.
	7	S Ursæ Majoris, M.
	9	L² Puppis, M.
	9·4	W Virginis, *m*.
	11	η Geminorum, M.
	11·5	β Lyræ, *m*.
	15	R Cassiopeiæ, *m*.
	15	R Vulpeculæ, *m*.
	17	U Monocerotis, M.
	17	T Geminorum, M.
	17·4	T Monocerotis, M.
	17·6	W Virginis, *m*.
	18·3	ζ Geminorum, *m*.
	20	R Scorpii, *m*.
Dec.	23	R Andromedæ, M.
	23·3	ζ Geminorum, M.
	24	W Ophiuchi, M.
	24	R Draconis, M.
	24·5	β Lyræ, *m*.
	25·3	T Monocerotis, M.
	26	R Lyræ, *m*.
	26	χ Cygni, M.
	26·7	W Virginis, *m*.
	27	S Leonis, M.
	27	S Canis Minoris, M.
	28	T Herculis, *m*.
	28·4	ζ Geminorum, *m*.
	31	R Coronæ, M.

Variables of Short Period.

δ Cephei.	d.
Dec.	6·55, M.
	10·35, *m*.
	15·65, M.
	17·25, *m*.
	22·65, M.
	26·45, *m*.
	31·75, *m*.

U Coronæ, *m*.		h	m
Dec.	5	14	0
	12	11	43
	19	9	25
	26	7	8
	29	17	59

η Aquilæ.	d.
Dec.	4·4, *m*.
	6·8, M.
	11·6, *m*.
	21·2, M.
	28·3, M.

U Cephei, *m*.		h	m
Dec.	3	15	7
	8	14	46
	13	14	26
	18	14	5
	23	13	44
	28	13	24

Algol, *m*.		h	m
Dec.	1	14	9
	4	10	58
	7	7	46
	21	15	51
	24	12	40
	27	9	29
	30	6	18

λ Tauri, *m*.		h	m
Dec.	1	8	9
	5	7	1
	9	5	54

X Sagittarii.	d.
Dec.	1·3, *m*.
	8·4, *m*.

δ Libræ, *m*.		h	m
Dec.	1	8	23
	3	16	14
	8	7	57
	10	15	48
	15	7	31
	17	15	22
	22	7	5
	24	14	56
	29	6	39
	31	14	30

S Cancri, *m*.		h	m
Dec.	2	5	14
	11	16	52
	30	16	8

Astronomical Memoranda, December 1885.

Equation of Time:—Sun *before* Clock, Dec. 1, $10^{m}\ 40^{s}$; Dec. 11, $6^{m}\ 23^{s}$; Dec. 21, $1^{m}\ 31^{s}$; Dec. 31, *after*, $3^{m}\ 24^{s}$.

Sidereal Time at Mean Noon:—Dec. 1, $16^{h}\ 42^{m}\ 1^{s}$; Dec. 11, $17^{h}\ 21^{m}\ 27^{s}$; Dec. 21, $18^{h}\ 0^{m}\ 53^{s}$; Dec. 31, $18^{h}\ 40^{m}\ 18^{s}$.

Sun.	Rises.		Sets.		Position-angle of axis.		Heliogr. co-ordinates of centre of disk. Lat.		Long.	
	h	m	h	m	°	′	°	′	°	′
Dec. 2....	19	48	3	52	+15	28	+0	26	234	52
7....	19	54	3	50	13	24	−0	12	168	59
12....	20	0	3	49	11	13	0	50	103	7
17....	20	5	3	49	8	56	1	28	37	14
22....	20	7	3	51	6	34	2	6	331	21
27....	20	8	3	54	4	9	−2	42	265	30

The Moon

	rises.			*sets.*			*sets.*			*rises.*	
	h	m		h	m		h	m		h	m
Dec. 1	14	53	Dec. 9	6	51	Dec. 17	15	24	Dec. 25	9	0
2	16	3	10	7	49	18	16	36	26	10	16
3	17	10	11	8	49	19	17	49	27	11	31
4	18	16	12	9	51	20	*rises*		28	12	44
5	*sets*		13	10	53	21	4	10	29	13	55
6	4	22	14	11	58	22	5	13	30	15	2
7	5	7	15	13	4	23	6	25	31	16	10
8	5	57	16	14	13	24	7	42			

Dec. 6^{d} 1^{h} 17^{m} New Moon. Dec. 14^{d} 6^{h} 22^{m} First Quarter.
21 8 59 Full Moon. 28 0 22 Last Quarter.

Planets for December 1885.

Day.	Planet.	R.A.			Dec.			Diam.	Rises.		Tr.		Sets.	
		h	m	s	°	′		″	h	m	h	m	h	m
1	Mercury..	18	3	17	25	39	S.	6·6	21	42	1	21	4	57
11		18	22	6	23	43	S.	8·8	20	57	1	0	4	55
21		17	36	2	20	42	S.	9·6	19	16	23	26	3	48
31		17	14	37	20	13	S.	8·0	18	16	22	32	2	49
1	Venus ..	19	56	45	23	31	S.	22·8	23	21	3	15	7	8
11		20	39	46	20	46	S.	25·4	23	5	3	18	7	29
21		21	17	57	17	22	S.	28·8	22	43	3	17	7	48
31		21	50	8	13	36	S.	32·8	22	13	3	10	8	3
1	Mars	10	53	7	9	29	N.	8·3	11	17	18	9	1	4
11		11	8	53	8	6	N.	8·9	11	1	17	45	0	33
21		11	22	39	6	54	N.	9·6	10	41	17	20	0	0
31		11	34	2	5	57	N.	10·5	10	18	16	52	23	26
1	Jupiter ..	12	10	30	0	9	N.	32·1	13	21	19	26	1	33
11		12	15	11	0	19	S.	32·9	12	48	18	51	0	57
21		12	19	0	0	41	S.	33·9	12	15	18	15	0	19
31		12	21	51	0	56	S.	34·9	11	40	17	39	23	38
1	Saturn ..	6	29	41	22	23	N.	18·4	5	36	13	45	21	55
31		6	19	30	22	32	N.	18·6	3	26	11	37	19	48
1	Uranus ..	12	26	34	2	6	S.	3·6	13	48	19	42	1	40
31		12	29	16	2	23	S.		11	54	17	46	23	38
1	Neptune..	3	27	20	16	59	N.		3	9	10	43	18	17
31		3	24	33	16	50	N.		1	10	8	43	16	16

Dec. 8^d 19^h. Venus at greatest elongation (47° 16′ E.).
9 3 Mercury stationary.
18 16 Mercury in inferior conjunction.
25 23 Saturn in opposition.

Phenomena of Jupiter's Satellites, December 1885.

d	h	m		d	h	m		d	h	m	
3	17	9	iii. Tr. I.	14	14	29	iii. Oc. R.	22	15	14	i. Oc. R.
4	14	8	ii. Tr. E.		15	59	i. Tr. E.	26	13	20	iv. Tr. E.
5	17	19	i. Tr. I.	16	19	2	ii. Ec. D.	27	16	10	ii. Oc. R.
6	16	56	i. Oc. R.	17	16	36	iv. Ec. D.		19	10	i. Ec. D.
7	14	4	i. Tr. E.	18	16	36	ii. Tr. I.	28	14	31	iii. Ec. D.
9	16	27	ii. Ec. D.	20	13	35	ii. Oc. R.		17	31	i. Tr. I.
	17	26	iv. Tr. I.		17	17	i. Ec. D.		17	31	iii. Ec. R.
11	13	59	ii. Tr. I.	21	13	34	iii. Ec. R.	29	13	38	i. Ec. D.
	16	47	ii. Tr. E.		15	35	iii. Oc. D.		17	7	i. Oc. R.
13	15	24	i. Ec. D.		15	38	i. Tr. I.	30	14	15	i. Tr. E.
	18	51	i. Oc. R.		17	54	i. Tr. E.				
14	13	43	i. Tr. I.		18	30	iii. Oc. R.				

Occultations for December 1885.

Day.	Star.	Mag.	Disap.	Angle.	Reap.	Angle.
			h m	°	h m	°
2	κ Virginis	4½	16 24	129	16 56	190
11	18 Aquarii	6	8 31	130	9 30	267
12	B.A.C. 7697	6½	5 56	74	6 56	332
16	μ Piscium	5	13 47	120	14 40	271
17	B.A.C. 741	6½	15 9	28	15 23	357
18	B.A.C. 987	6½	11 2	138	12 2	251
20	111 Tauri	5½	17 6	72	17 56	287
20	117 Tauri	6	18 35	40	19 6	319
21	B.A.C. 1930 ...	6½	5 46	78	6 37	294
24	ξ Leonis	6	15 55	69	17 5	252
25	48 Leonis	6	18 50	93	19 48	223
26	τ Leonis	5	17 48	100	18 47	211
28	θ Virginis	4½	16 5	85	17 15	226
31	η Libræ	6	17 8	89	18 15	234

The R.A. and N.P.D. of Planets and the Solar Co-ordinates are given for Greenwich Mean Noon; in all cases Greenwich Mean Time is used, reckoning from noon to noon, as in previous years; the angles for Occultations are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

THE OBSERVATORY,

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No. 104. DECEMBER. 1885.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, November 13, 1885.

E. Dunkin, F.R.S., *President*, in the Chair.

Secretaries: E. B. Knobel and
Lieut.-Col. G. L. Tupman.

The preliminary business of the Meeting concluded,

Mr. Knobel read "Notes on the Total Solar Eclipse, 1885, September, observed in New Zealand," by *C. R. Marten*. These notes were not claimed to be complete observations; but as the weather was unfavourable for the official observations, Mr. Marten thought his notes, containing certain details in which he took a personal interest, might be acceptable to the Society. The observations were taken at a station 100 feet above sea-level. An ordinary binocular field-glass was used, with an excellent chronograph and self-registering thermometers. At $7^h\ 4^m$ A.M. the Sun appeared through a rift in the cloud, and after that an absolutely perfect view of the eclipse was obtained. No noteworthy diminution of light was observed until about two thirds of the solar disk had been covered, and then the light faded away rapidly. As totality approached, the shadows were weird and striking. For about 15 or 20 seconds, the entire disk of the Moon became suddenly visible against the white background of the solar corona. An instant before totality the solar light was centred into one intensely brilliant point, like the beam from an electric arc, and then came complete obscuration. The darkness resembled that of a moonlight night. This he attributed to bulky masses of cloud on the horizon, and partly to the influence of the glow which surrounded the eclipsed sun like a halo. The corona presented a splendid spectacle, with streamers of two lunar diameters or more in length. A long ray proceeded from the lower right quadrant, and there was apparently an equatoreal protuberance resembling the Aurora Australis. There was an unsteadiness and a varying degree of luminosity noticeable in the coronal rays. The red prominences had been visible just before with a remarkable brilliancy to a far greater extent than he had ever seen described. The prominences were rich in colour, some resembling the corona, and varied in depth of hue from pink and rose to scarlet and crimson. This could be seen with the naked eye. There was a fall of temperature of 5°·5 during the eclipse; and the disturbed state of the atmosphere might perhaps be responsible for some variation and unsteadiness in the coronal light and red flames.

Mr. Ranyard did not think it necessary to assume that the colours observed were due to any exceptional conditions, as dust from Krakatoa. The fact that the light had to travel through such a thick stratum of atmosphere would, in his opinion, account for the weird effects. The blue or short rays being absorbed, we should have all the usual sunset effects. The atmosphere during eclipses is generally very disturbed; as the shadow cone, which is a region of cold, sweeps over the observer, great changes take place, and currents are set up, and no doubt such currents cause disturbances.

Mr. Penrose. In observing the eclipse of 1878, I noticed something which bears very much on this unsteadiness. There was a very definite bundle of streaky filaments at the lower edge of the disk, and the ends of these appeared to me to tremble.

Lieut.-Gen. Tennant. Does Mr. Martin mention anything about these waves of shadow which many observers have seen pass across the fields? they sometimes have been called colour-waves. I think, if you will refer to the eclipse of 1868, General Addison, at Aden, saw waves or bands of light passing across the walls. Where do these bands come from? They appear as bands of light near totality, but one cannot understand them.

Mr. Ranyard. There is evidence that they do exist; there are 30 or 40 observations. They may be accounted for when it is remembered that the chief part of the light comes from the horizon through our disturbed atmosphere. The illumination is very peculiar. Some persons have attempted to observe whether a shadow is cast by the corona; but the illumination from the horizon overpowers the light from the corona, and the latter appears to vanish, and therefore you have the bands of light from the light upon the horizon coming slantingly along the surface of the earth.

Lieut.-Gen. Tennant. General Addison saw them on the walls.

Mr. Ranyard. They would be better seen on the walls than on the ground, because if from the horizon, they would fall perpendicularly upon them.

Lieut.-Gen. Tennant. But you cannot have rays of light from the dark band. If you were to attribute it to a nervous feeling in the observer's eye, consequent on the change of light, I could quite understand it; but I find it difficult to understand it as a real phenomenon that comes from the horizon.

Mr. Penrose. I have seen the shadows running on the ground, but it was after the reappearance of the Sun.

Mr. Klein. This apparent movement of shadows may be a purely optical illusion, due to the fact that the last portion of the Sun visible just before totality, being a mere point of intense light, all shadows are rendered momentarily more visible by their sharper outlines than when, before eclipse, a larger surface of Sun-disk illumination and the consequent increase of cloud reflection toned them down.

Mr. Ranyard. I do not know that I follow that. The source of light is a large surface. It cannot be due to nervous effect, for different observers all see the bands alike and moving similarly.

Mr. Stone. In the only eclipses I have seen, the air was exceedingly still, and I saw none of the tremors which have been spoken of, but there were no clouds in the neighbourhood. I think that some of these disturbances are entirely atmospheric; at the same time there is marked appearance of change in the aspect of the country as totality is coming on; and there was a nervous creeping coming over everybody. I can understand that if imagination was allowed play much might be suspected, but certainly no change took place in the corona.

Lieut.-Gen. Tennant. I cannot conceive it to be imagination; the fact remains. How it is to be accounted for, I cannot say.

The President. So far as my experience goes in observing the total eclipse of 1851, I noticed no tremor whatever in the telescope during the time of the totality. The images of the Sun, the prominences, and the corona (so much as was visible) were perfectly steady; with regard to the shadow, it was seen travelling onwards from west to east, and that all my assistants noticed; and while my eye was at the telescope, one of those gentlemen said, "I can see the shadow distinctly coming on;" and afterwards it was seen by myself to go off. We are very much obliged to Mr. Marten for communicating this observation, as we may, perhaps, have no other notices of this eclipse from that part of the world.

Mr. Knobel read a paper by *Professor Pritchard*, "On Photometric Observations of the Nova in the Andromeda Nebula observed at Oxford." The star was first observed on Sept. 7th, when the brightness was measured at 8·25 mag. The star declined till the end of September, when it was about the 10th mag., at which it continued till Nov. 12. But there was considerable difficulty in observing the brightness of this new star on account of the brightness of the nebula in the background. Owing to the small area of the wedge, there was, near the point of extinction, some trace of unextinguished light from the nebula. From some cause or other the behaviour of the star in the nebula, as it approached extinction, was somewhat different to an ordinary star; but in the large refractor there is no perceptible difference between the Nova and other stars of similar brightness.

Mr. Knobel read a note by *Father Perry* on "The Outburst in

Andromeda." The nebula had been observed at Stonyhurst from Sept. 13 to Nov. 18; on Sept. 13 the spectrum was continuous at the red end, and there was a decided maximum of brightness in the green: a bright band in the green was suspected, but not clearly seen. On Oct. 9th the spectrum was still brightest in the green. There was no great change from day to day, the magnitude remaining about 11. With a low power the nucleus alone was visible; with powers of 200 and 300 the Nova was more brilliant than the nebulæ.

Mr. Maunder read a paper on "Observations of the Spectrum of Nova Andromedæ made at the Royal Observatory, Greenwich." He had observed the spectrum on Sept. 4 with the "half-prism" spectroscope and simply saw a continuous spectrum, extending from D to F, and without any trace of lines bright or dark. The dispersion employed may have been too great, for on Sept. 11 with the "single prism" spectroscope, he was able to see what appeared to be two bright lines and he got rough measures of their places. On the same night he examined two stars near the Nova in order to see whether the comparative faintness of the red end of its spectrum was due to the feeble light of the star or to anything in the spectroscope, or was really characteristic of the spectrum. The result of the comparison showed that the red end was proportionally fainter in the spectrum of the Nova than in that of a star of about the same magnitude. On Sept. 15, with a prism held before the eyepiece of the Great Equatoreal, two bright points where again suspected. On Sept. 30, with the half-prism spectroscope reversed, the lines were seen more distinctly, and a third line was suspected. One of the lines he believed he could trace at times into the nebula; but it was very difficult to keep the nebula on the slit, which had to be very narrow, and he could not satisfactorily confirm his suspicion. The three lines, the measures of which had been confirmed by observations made at Yale College, agreed in position within the limits of error of observation with the three coronal lines 1474, 1350, and 1250 of Kirchhoff's scale. The spectrum of the Nova was traced towards the violet very little further than that of the nebula; but towards the red it was visible considerably further. It might be noteworthy that though no lines could be distinguished at C, D, F, or G, yet the spectrum of the Nova began about C and terminated at about G, and its brightest portion began at D and ended at F.

Mr. Turner remarked that Prof. Pritchard communicated his magnitudes to Greenwich, and at the time the general impression was that the star was brighter than he had measured it, and that is borne out by Mr. Maunder's remarks about the magnitude of the star on the day he observed the spectrum.

Mr. Knobel. I quite agree with Mr. Turner that the photometric comparisons probably make the star fainter than it appears to the eye. I have made observations from September 3rd up to last Saturday, when I last saw it. I made 12 or 14 extinctions on each night that I had the chance of seeing it, and I have come to the conclusion that all the extinctions I have made on the principle of limiting apertures make the star too faint. I have formed an opinion as to why that is, and I believe it will apply also to observations made by the wedge-photometer. The star becomes invisible to the eye when it cannot be distinguished from the nebula which forms the background of the star, and the aperture which makes it invisible in every case seems to me much too large, and therefore makes the star much too faint relatively to the comparison star which has been used in the observation. I disagree with Professor Pritchard in one statement he makes, where he says that "about the end of September this star approached closely to the tenth magnitude, to which it has continued constant to the present day." I do not think that it is so. I observed it carefully last Saturday, Nov. 7th, cloudy weather having prevented me observing it at all since Nov. 1, when I carefully estimated it as three quarters of a magnitude brighter than the small star that precedes it 112″ or 114″. On Nov. 7th it had decreased in brightness from what it was on the 1st, and it was in my estimation about half a magnitude brighter than the small star. But the stars were too faint for the observations to be made by the method of limiting apertures. I would like to ask Mr. Maunder whether, on Sept. 4th, when he made his first observations, they were made with a spectroscope of rather too high a dispersion, because when Dr. Huggins made his observations on Sept. 3rd he found with a too high dispersion he was unable to see any indications of bright lines, but by using a spectroscope with a lower dispersion he was able to see unquestionably indications of bright lines.

Mr. Stone. Does Dr. Huggins say unquestionably? Was it not rather a supposition of his?

Mr. Knobel. No; I had a letter recently and it was stronger than that—he was satisfied *.

Mr. Stone. The only reason I ask is this: I think a star of the 7th magnitude should show clearly whether the spectrum is or is not linear. I am judging from observations of T Coronæ which I made. There is no doubt whatever about the spectrum having been linear in that case until the star was below the 8th magnitude, then it was a doubtful thing to say whether it was a linear spectrum or not. Unless you know where to look for the line I should think it is exceedingly difficult to determine whether it is a linear spectrum in the case of a star below the 8th magnitude, but certainly it is not difficult with a star of the 7½ magnitude. I am merely judging from that one star.

Mr. Knobel. That star was very much brighter than the one now in question.

Mr. Stone. I watched it as a linear spectrum until it was considerably below the 8th magnitude, and I could distinguish it as a linear spectrum below that. I think, therefore, if this star had a proper linear spectrum and any one had had an opportunity of observing it with a glass of 15 inches when it was of the 7½-magnitude, there ought to be no doubt about the character of the spectrum. I am simply judging it from the only case I have thoroughly tested, the case of T Coronæ.

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* The word "satisfied" was applied only to observations made on Sept. 9.—E. B. K.

Mr. Knott remarked that by rough estimate it was about 8½ to 9 mag. on Sept. 28; but as this was a hurried observation, on account of clouds, it was probably too high, for on Oct. 1 it was only about the 10 or 10½ mag. Its colour was then whitish, and its disk rather large and undefined. Gauging by the method of limiting apertures, he found it about the 11th mag. on Oct. 27. On Oct. 27 he also gauged the star referred to by Mr. Knobel, and found it about 12·4 mag. On Nov. 7th, the same night when Mr. Knobel was making his observations, the Nova was 11·8, while D'Arrest's star was gauged 12·3. The difference of ½ a mag. between these two exactly agrees with Mr. Knobel's observation. He further stated that he felt great difficulty in gauging the magnitude for the same reason that Mr. Knobel has stated, viz. the brightness of the background on which the star was projected. He had gauged it with a power of 191. His observations thus agreed with Mr. Knobel's in the fact that the star decreased in magnitude considerably between October and November, whilst they were hardly in accordance with Prof. Pritchard's views.

Mr. Turner. There was also a general impression with the transit-circle observers at Greenwich that the star was decreasing rapidly throughout October. The observations became much more difficult towards the end of the month. With regard to Mr. Knobel's remark about the photometric observations as opposed to eye observations, it appears to me the question is similar to that of the moonlight background, as to whether the background does or does not interfere with the accuracy of wedge-photometer.

Mr. Brett. I would like to call attention to the unreliable character of wedge photometry. Most observers cannot be depended upon below the 8th degree of magnitude. I know many good observers who will see a 9th magnitude star go out when I can see it long afterwards. The eyes of some persons are excellent for high illumination, and the eyes of other people are good for low illumination, and there are many people who cannot correctly estimate degrees of light below the 8th magnitude.

Mr. Stone. I should like to say a word not in proof of a theory I brought forward some years ago, but to explain its possibility. The Sun is known to be surrounded by a great deal of coronal light. Similar envelopes must surround the stars generally. These gaseous envelopes might, in the case of a cluster, form over the whole or a part of the cluster one mass of gas. So long as such a cluster was within a certain distance of us, the light from the stars would prevail over the light from the gaseous envelope, and an ordinary stellar spectrum would result. Suppose such a cluster removed further and further from us, the light of the stars would diminish inversely as the square of the distance, while the light from the envelope would remain sensibly constant, because the contributing area would be increased in the same proportion that the light from each part was diminished. The result would be that for distances beyond a certain limit the chief light received would be nebular and not stellar, and the spectrum would become a linear one.

Mr. Common remarked that, with regard to a star being veiled with nebulous matter, a sufficient magnification would detect the difference between a nebulous mass which you can magnify and a star which you cannot magnify. And whilst an increase in the magnifying-power brings more stars into view, it decreases the visibility of nebulæ.

Mr. Ranyard. I saw the star for the first time on the 4th, and was not able to see any bright lines in its spectrum. If there were bright lines in it they were very faint as compared with the rest of the spectrum.

Mr. Maunder. The bright lines were certainly very faint as compared with the linear spectrum; they were only just distinguishable on it. I think my failure to see them on Sept. 4 might possibly be due not only to the fact that I used a greater dispersion than on Sept. 11, but also that I did not use so narrow a slit. I also tried a cylindrical lens on Sept. 4th. On the other hand, it might be that the relative brightness of the linear and the continuous spectrum changed from one date to another. Of course we cannot assume that the relative brightness of the bright line spectrum and of the continuous spectrum was always the same either in this star or in T Coronæ.

A vote of thanks was then passed to the authors of the papers.

Mr. Hilger said that he wished to bring before the Society a new form of governor for the driving-clock of an equatoreal. He had often made Foucault governors, which used to be considered the best for driving-clocks, and the present one resembled those so far as the fans were concerned. But he did not believe in any form of governor which depended upon friction; and in this form the friction was confined to four fine points which carried the two fans, and he had succeeded in bringing the action under such perfect control, that it was possible to take off or put on an additional 140 lbs. to the driving-weight without any appreciable difference in speed. The fans were pulled towards the spindle by means of chronometer springs. There was no parallelogram motion to keep the fans inclined at equal angles to the spindle, and yet, under every variation of position, they always opened symmetrically; when he first tried the fans, he found they worked at different speeds when close to the cylinder than when they were open. That difficulty he overcame in a manner he proceeded to explain. He would be most happy to show the practical working of the clock, and explain any further details if any one in the room desired further information.

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Mr. [illegible]. There is one objection to this clock. The wire [illegible] and when it comes to the end of the coil it will wind upon a different diameter, which will make a difference in the driving-power of the clock, and it will also have a great tendency to wear out the wire. With regard to the rubbing [illegible], to which Mr. Hilger objects, I have no objection to them. My chief objection is to the irregularity of the larger driving-wheels, as it is very difficult to get the teeth exactly equal in the large driving-wheel.

A vote of thanks was then passed to Mr. Hilger.

Mr. [illegible] read a paper on "Erratic Meteors."

Mr. Proctor. These zigzag paths are analogous to the path a thin piece of slate or a boomerang thrown with a high velocity into the air would follow, owing to the peculiar form of the material.

Mr. Ranyard. I think the curved paths are well known. I remember 10 years ago Professor Newton, of America, accounted for them by the irregular shapes of the meteoric particles.

The President. Many of you have seen one of the French Star Charts passed round the room, and you will see what very interesting results have been obtained in celestial photography in Paris, and also by Mr. Common. M. Mouchez considers that the experiments having been so successful, it is important that some action should now be taken to have some regular course of observations made in different parts of the world, at different observatories, so as to take photographs of the whole of the heavens. He has forwarded to me a letter which I think you would understand better in French than you would a mere translation. [*The President* then read the letter in French.]

The President continuing said: Of course what M. Mouchez wishes is that six or eight observatories in different parts of the world should combine and make these photographic observations on similar principles as Mr. Gill is already doing at the Cape. Six observatories should combine to carry out this scheme systematically, so that if four more—say, one in Australia, and another in America, and another perhaps in England, and another somewhere else—take the matter up, we shall have the number required. It would be very important indeed if this could be carried out.

The President here announced that the Meeting would now proceed to ballot.

Mr. Common. What about the discussion on this clock?

The President. Mr. Hilger has not explained its construction. I do not think we know anything to discuss without further explanation.

Mr. Common. I think it is Mr. Hilger's duty to explain it. He stated that this is the best clock in the world, and that it would give perfect accuracy. With regard to what has been said with regard to the photography of the heavens it all depends upon a good clock. Therefore I think it will be interesting to know all about this clock. I have very strong doubts about it as to whether this clock would drive in the way Mr. Hilger says it would. I ther believe in a clock controlled by friction. This clock relies n the friction of air instead of against a solid base or on a wheel. I cannot see that the springs which Mr. Hilger has enclosed in these two boxes have that proper relation to the angle that these fans take as to give that necessary control which Mr. Hilger claims. I do not wish to criticise this clock in any contrary spirit, but only to find out the best clock for driving a telescope.

Mr. Turner. Through the kindness of Mr. Hilger I was allowed to see the clock at his workshop, and he put it through a series of tests. One test he subjected it to was the constancy of the driving with different weights. The clock is driven with weights varying from 120 lbs. to 240 lbs., and the constancy of rate was practically tested for those two weights. It was tested by a counter which revolved once in a minute and a half. Originally the clock was found to drive more slowly, by about six tenths of a second, with the heavy weight. Then Mr. Hilger altered the position of the small weights, which he claims to be the adjusting feature of the clock, and that difference was reduced to three tenths of a second. He then adjusted it a little further, and finding he could not adjust it perfectly accurately without more time, he thought it would be better to show it could be over-adjusted, *i. e.* made to drive quicker when the weight was off, and that was done to the extent of four tenths or five tenths of a second per revolution. You could not, of course, make it drive still more quickly when all the weight was taken off; but it was practically found that the rate of the clock could be over-corrected at those two points, and consequently there was a strong presumption in favour of the possibility of regulating the clock to drive throughout the intermediate range with an accuracy approaching the limit of observation. The observations of two observers agreed generally within one tenth of a second.

Mr. Penrose. Was it doing any work?

Mr. Turner. No; but the driving part of the clock was itself varied.

Mr. Ranyard. The great difficulty is to get an even rate.

Mr. Common. I would like to remark that the working of these clocks, however well you may regulate them, depends entirely upon the strength of the spring, and that spring is affected by the temperature. The temperature is an all-important consideration. I have adjusted a clock I have with my 3-feet telescope to almost absolute accuracy. I am sure the rate was regular enough, but I found as the night grew colder, so the clock began to gain, and the colder it got the more it gained, and here we have no compensation for temperature at all. There is no doubt at all that a good clock is of the greatest importance, especially to people engaged in photography, and I have thought of a great many ways and made a number of experiments of very peculiar kinds. If you have a very heavy pendulum swinging at one or two seconds, so that a little motion and very fine spring will not affect it you will find you g a margin of effort in this fine spring, and you get a controlli power; and if you have a spring attached to that rod, and attached to a crank, and also to a pendulum, you will find that the pendulum will give you a very fair motion. I should think it is one of the most useful lines to go upon, and is far better than anything that is controlled by a spring, because you can compensate the pendulum but not the spring. If you have to have the clock where the telescope is, and you found the temperature falling from 55° to 30° or 25°, or even to 20°, as I have known it, you will find an enormous difference in the rate of the clock. I do not want to make any objection to this clock as a clock, but I think it is important, in threshing this subject out, that the best governor should be obtained, and I do not think Mr. Hilger is working upon the right lines in having a spring which is liable to change the governing power owing to the temperature.

Mr. Hilger. I tried the clock under different temperatures for that very reason, and when the fans are wide open it makes no difference if the temperature varies because the fans will come close together, but the speed will keep the same, whether the air is dry or moist.

The Astronomer Royal. It seems to me that the important point of this clock is that when you take the weights off you make the clock go faster. I should like to ask Mr. Common if he has seen any clock in which that could be effected, because that is an important point. With an increase in friction the clock may be made to go faster, or it may be made to go slower, and therefore it may be made to go at the same rate, whatever the friction may be. The difficulty that I have seen in the case of other clocks is that they vary with the load they have to drive.

Mr. Ranyard. There need be no variation of the driving-power if you have a long driving-cylinder.

Mr. Common. I do not see that anything would be gained by having a clock that would do either one thing or another with the weight taken off.

The Astronomer Royal. You want to have the power to make it go faster as well as to go slower, then you have the power of making it go at the same speed.

Mr. Common. So you can if you can make it go slower. The only objection I make to this clock is that where you have a spring which is acted upon by the temperature your error will be very much greater than with a pendulum; and hence I say the best line to go upon in making a driving-clock is that upon which Sir George Airy went at Greenwich, namely, a conical pendulum. This clock with a spring will go very well if it is kept in a mean temperature room, but if you apply it to the rough test of actual work you will find you want compensation. If you want to trust the clock entirely, you must have something in the shape of a pendulum to compensate for the difference in temperature.

Mr. Buckney. With reference to M. Villarceau's flying governor, the flyers are kept in position by the weight sliding on the central spindle moving up and down as the fans open, and that seems to be free from the objection which Mr. Common has urged, that the force of the spring would vary with the temperature in this governor. The rate of the clock when the flyers were in different positions was adjusted in much in the same way as by adjusting the weights described by Mr. Hilger, and I presume they are similar. The weights were adjusted on screws, and these weights you could adjust so that the clock would go at the same rate when the motive power was varied.

The following papers were also announced:—

T. Mackenzie. "On the Application of a Nicol's Prism to Sextant Observations."

J. Maguire. "Total Solar Eclipses 878 to 1724."

Capt. W. Noble. "Daylight Occultation of Aldebaran on July 9th, 1885.

A. A. Rambaut. "To reduce the Precessions computed with Bessel's constants to an agreement with those of Struve."

Rev. T. E. Espin. "Note on Mr. Chambers's suspected new Variable in Corona Borealis."

A. Marth. "Ephemeris for Physical Observations of Mars, 1886."

Prof. C. Pritchard. "On Dr. Wilsing's Examination of the Wedge Photometer."

The Rev. S. H. Saxby was duly elected a Fellow of the Society.

The Meeting adjourned at $10^{h}\ 0^{m}$ P.M.

The Greenwich System of Sympathetic Clocks and the Distribution of Time-signals.

[Continued from p. 369.]

For a glance at the actual working of the system we will imagine the time is nine o'clock in the morning. A clock-star is selected from the observations of the previous evening, its transit *read off* the chronograph-barrel, the mean of the several wires taken and reduced to the centre wire, and the proper instrumental corrections * applied. The result gives the time shown by the sidereal standard at the instant of the star's meridian passage, the difference between which and the tabular R.A., as obtained from the 'Nautical Almanac,' gives the error of the S.S. clock as obtained by the particular observer. But the times of transit, as estimated by the several observers, differ by a small quantity; hence the necessity, where there is more than one observer, to choose one as a standard to which all the others are referred. When this *personal equation*, as it is termed, has been applied the resulting error is compared with the error found on some previous occasion, and the difference regarded as the *rate* of the clock during the interval. When the proportional part of this rate is applied to our error as found by the star, we know the clock's error at the present time. A comparison is carefully made between the sidereal chronometer A, and the mean solar chronometer B, by waiting for a coincidence of beats. The difference between the two is then augmented by the *acceleration* of the sidereal clock on mean time to reduce to mean noon. To this is added the S.S. time at mean noon, and whatever the result differs from $0^h\ 0^m\ 0^s$ is the error of the mean solar clock. Suppose the latter is found to be $0^s{\cdot}5$ slow, the handle of the commutator is turned to A for five minutes. Another comparison is made before ten o'clock to correct any outstanding error.

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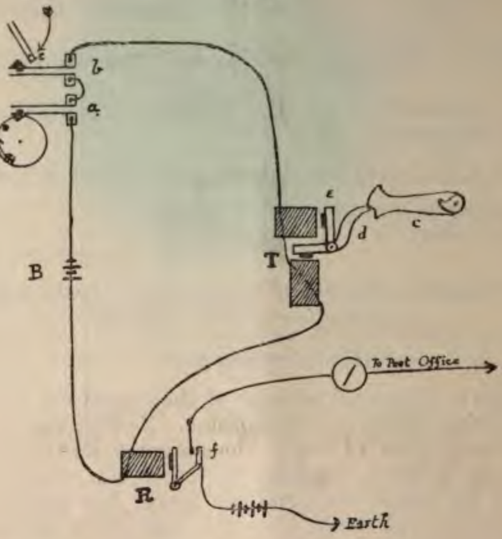
* Collimation, level, and azimuth.

While waiting for the ten o'clock signal to go out, let us glance at the Post-Office circuit, a plan of which is given in fig. 3.

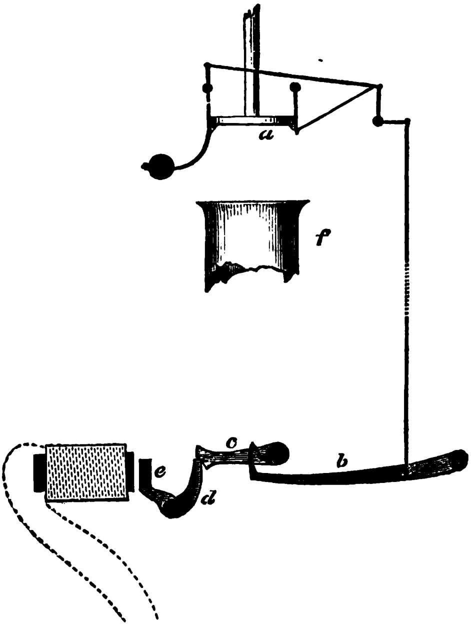
Fig. 3.



First, the circuit B, *a*, *b*, T, R, is the hourly signal circuit, through which a current passes every hour. B is the battery, R the Post Office or hourly signal relay, T the ball-trigger magnets, and *a* and *b* springs in the mean solar clock. A pin (shown by the dot) on an arm *c*, fixed to the axis of the seconds hand closes the springs *b* at the 0^s of every minute, while the springs *a* are closed from $59\frac{1}{2}^m$ to $60\frac{1}{2}^m$ each hour by a pin on a disk turning with the minute hand. We see therefore that both springs are closed every hour at the same instant, and the circuit is completed. The current passing through the relay R closes the springs *f*, and the signal passes out to the Post Office, whence it is sent in all directions to every part of the kingdom.

In fig. 3 will be noticed the coils T in the hourly signal circuit. These are used to drop the Greenwich Time ball, the principle of which is seen in fig. 4. The ball slides on a mast fixed on the east turret of the Observatory. A rod, sliding in a groove of the mast, and attached to the ball, terminates at its lower end in a piston *a* which rests at the bottom of an iron cylinder *f*. This rod is raised by means of a winch and chain; the latter passes over a pulley and is brought down to a movable collar sliding on a couple of guide-rods parallel to the ball-rod. When the chain is wound on the winch the collar rises and lifts the piston to the position shown in fig. 4, where it is maintained by two clips. The collar

Fig. 4.



slides down again on unwinding the chain. These clips are connected by a series of levers with the lever *c*, which is, some minutes before the hour, lifted into the position shown in the diagram, where it is held by the arm *d*.. The hourly current passing through the coils T, the arm *e* is pulled, and with it the support *d*, causing the levers *c* and *b* to fall. In consequence of this, the clips are opened outwards, leaving no support for the piston *a* which at once descends, bringing the ball with it. The ball slides down the mast at first rapidly, but the compressed air in the cylinder, acting as a buffer, brings it gently to rest. The instant of leaving the cross at the top of the mast is the time to be noted. The ball is only dropped at 1^h every day, but, as is seen from the figure, it could be dropped at any hour, since the relay is in the hourly circuit.

It may be as well to mention here that the Westminster clock is not directly controlled by the Greenwich current. By an arrangement in the clock, two signals are sent along the Post-Office wire to the Observatory every day at certain times. Should these signals show an error of 3 or 4 seconds, notice is sent to Messrs. E. Dent and Co., who at once take measures to correct it. This is seldom necessary. The performance of this clock is always carefully exhibited in the Astronomer Royal's report. At the present time all signals sent from the Observatory go first to the General Post Office. Previous to 1884, Feb. 29, signals were transmitted hourly to the S. E. R. Co.'s station at London Bridge. With the exception of the signal at 1^h, these were at the disposal of the company, who for this privilege allowed the 1^h current to pass along their wire to Deal for the purpose of dropping a ball on the old semaphore tower. Provision was also made for return signals, which are necessary to insure the proper working of the ball. Some time prior to 1884, Feb., the S. E. R. Co. informed the Astronomer Royal that they could not continue to allow the current to pass, as the wire was in constant requisition. Arrangements were then made to drop the ball through the medium of the Post Office (see fig. 5).

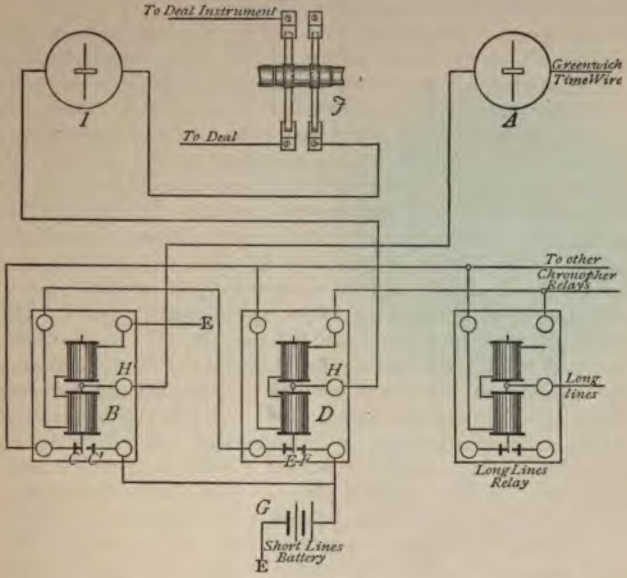
The time-signal work has increased so much, that at present there are two chronophers in use at the office in St. Martin's-le-Grand. These chronophers consist of numerous switches and relays, which are brought into and put out of action at the proper moments by a good clock. This automatic action sends the single Greenwich current on numerous different wires, which may be grouped as follows:—

1. Metropolitan. Signals every hour. These wires being used only for time-signalling remain always in connection with the relay.
2. Short Provincial. Signals at 10^h and 1^h: *e. g.* Portsmouth.
3. Medium Provincial. Signals at 10^h and 1^h: *e. g.* York.
4. Long Provincial. Signals at 10^h and 1^h: *e. g.* Edinburgh.
5. Deal Ball. Signal at 1^h.

In the normal state the provincial wires are connected with their respective speaking-instruments by vertical springs resting against small square studs. In the chronopher these springs are arranged in a row and crossed by a horizontal bar of iron. Shortly before the hour the clockwork forces this bar to rotate and push the springs away from the studs, while the bar is itself in connection with each spring. This bar, really in three parts, is also in connection with the several batteries through the medium of the relays, and hence a constant current is set up along all the wires warning the receiving stations. The effect of the Greenwich current is to set the relays in action and thereby reverse the current. This reversal is the time to be noted. The bar is then returned to its usual position.

As an example of the working of the chronopher, a plan of the arrangements for the Deal ball is given in figure 5. At the top will be seen the vertical springs and contact-studs with a portion of the bar which had to be insulated to admit of the return signal passing to Greenwich. Two extra relays were found necessary. The Greenwich current enters through the galvanometer A, thence to the tongue of relay B, along the movable tongue to C, thence to the second relay D, which effects the removal of the tongue from E to F, and so picking up a current from the battery G, which passes out to Deal through the tongue of relay D (leaving at H) through galvanometer I, and out to *line* by way of chronopher bar J.

Fig. 5.



The return from Deal enters through J and I to H of the relay D, and thence to E and the coils of relay B, removing the tongue from C to C′, and by this means picking up a current from the battery G, which passes by H through A to Greenwich.

The time lost in setting the relays in action and generating local currents is found in practice inappreciable, and full confidence may be placed in the signals thus automatically distributed. Besides these signals, however, the 10^h A.M. current from Greenwich is distributed *by hand contact* by clerks, who on hearing the signal simultaneously transmit signals by the usual speaking instruments. These go to some 600 offices not supplied by the chronopher, and these in turn distribute to others, and in this manner the Greenwich time becomes diffused throughout the United Kingdom.

The scale of charges may be found at the end of the Post-Office Guide. These charges vary with the distance of the receiver from the Local Head Postal Telegraph Office.

It is proper to state here that Sir G. B. Airy endeavoured for many years to secure an extension of the Time-signal System by the establishment of an hourly signal station at the Start Point. As far back as 1862 he succeeded in getting a Committee of the Board of Visitors to memorialize the Duke of Somerset, then First Lord of the Admiralty, in reference to the subject. After a mass of correspondence the Admiralty eventually declined, on the plea of cost, to erect and maintain a time-ball. In 1872 Sir George again brought the subject forward; but without success. The adoption of Sir George's plan would have entailed an original outlay of about £1800 and an annual cost of about £200. The present Astronomer Royal has taken up the matter again. He has received an assurance from the Committee of Lloyd's that that corporation would be willing to undertake the maintenance of hourly signals at the Start or Lizard, provided the government would supply the necessary apparatus. By this arrangement there would be no annual charge for maintenance, and it is estimated that the first outlay would not exceed a very moderate sum. As the objection to the original plan is thus cleared away, we may hope that before long ships passing down Channel will have the means of obtaining a good sea-rate for their chronometers. By the method proposed by the Astronomer Royal, the signals would be made automatically by a local clock, which clock would be corrected daily by the help of a time-signal from Greenwich at 10 A.M. This signal would start an auxiliary seconds pendulum suspended freely just behind the clock pendulum. The attendant would then make the clock pendulum pass through the middle of its vibration at the same instant and in the same direction as the free pendulum. This could be done by electro-magnetic action as described under the Mean Solar Clock. T. Lewis.

Est-il opportun de changer le Mode de compter le Jour en Astronomie?

La question ainsi posée a été soumise à un tour de préconsultation pendant la réunion de l' "Astronomische Gesellschaft" à Genève, au mois d'Août dernier. Son Président, M. le Prof.

Auwers, a introduit le sujet d'une façon magistrale à propos de la VI^e résolution votée par la conférence internationale de Washington en 1884.

Il s'est conformé au désir exprimé de divers côtés de le voir traité par des astronomes, et il a soigneusement annoncé que la conversation ne devait point aboutir à un vote, l'assemblée n'ayant point qualité pour en formuler, mais que les opinions de ses membres pourraient jouer un rôle dans l'histoire d'une motion récente et encore loin d'être mûre.

Un résumé des avis émis sur le sujet peut intéresser toutes les personnes s'occupant d'astronomie. Nous allons le tenter avant d'avoir pu prendre connaissance du procès verbal officiel de la séance, non encore publié.

Commençons par donner le texte de la résolution votée à Washington :—" VI. La conférence émet le vœu qu'on fasse commencer les dates astronomiques et nautiques dans le monde entier à minuit moyen, aussitôt que faire se pourra."

Le premier opinant a été M. O. Struve, Directeur de l'Observatoire de Pulkowa, et il a parlé avec l'autorité que lui confère sa grande position dans la science, en faveur du changement visé dans la résolution. On était averti de sa manière de voir par sa brochure sur les résolutions de la conférence, publiée au printemps. Il a développé la thèse énoncée dans cet opuscule, que dans le siècle de progrès où nous vivons, tout ce qui tend à simplifier les relations entre les divers peuples, les diverses carrières, mérite d'être sérieusement étudié et poursuivi. Les astronomes, ayant une façon différente de tout le monde de compter les dates, doivent y renoncer pour se conformer à l'usage général. Il a reconnu qu'il en résulterait pour eux un sacrifice, mais ce sacrifice devait être consenti pour ne plus faire bande à part, pour se plier à la méthode universellement adoptée, d'autant plus que les observatoires ont souvent affaire avec des institutions publiques, poursuivant des buts pratiques d'intérêts divers.

Les orateurs qui lui ont succédé ont très franchement soutenu le maintien du *statu quo.*

Le Prof. Spörer, de Potsdam, le zélé observateur des taches solaires, tout en se servant de l'heure civile pour ses travaux, ne réclame point de changement, se rendant compte des dangereuses conséquences que cela entrainerait pour les astronomes.

Après lui, le Prof. S. Newcomb, de Washington, développe ce point de vue avec beaucoup de force, prouvant que l'unanimité réunie à la conférence d'Amérique ne comprenait pas la voix la plus autorisée, la plus compétente, dans son propre pays, pour se prononcer en pareille matière.

La lettre insérée dans le cahier de Janvier écoulé des 'Monthly Notices' avait déjà prévenu le monde scientifique des vues conservatrices de M. Newcomb au point de vue aujourd'hui débattu. Il expose que depuis deux cents ans et au delà, les astronomes comptent le temps à partir de midi, sans qu'on ait jamais eu à se plaindre de leur méthode, dont ils se trouvent bien. Elle a pour eux des avantages positifs et leur laisse toute facilité pour passer au temps civil. Changer leur mode de faire entrainerait une confusion inextricable dans toute la littérature astronomique, et ferait courir la chance d'erreurs dans maintes recherches. Cette partie de son argumentation est traitée par l'orateur avec une "humour" fort goûtée de ses auditeurs.

Le Prof. Ed. Weiss, de Vienne, se demande à qui profiterait le sacrifice réclamé des astronomes, et il ne le découvre pas. Or lorsqu'on demande un sacrifice, il est raisonnable de s'enquérir en faveur de qui il doit être consenti. L'observateur se réfère toujours au méridien du lieu où il opère, qu'il compte en temps moyen, en temps vrai, ou en temps sidéral. L'unification des méridiens n'est point ici de mise, et jusqu'en éternité ce sera la nuit qu'on observera les étoiles. Il serait fâcheux d'avoir à introduire un changement de date au milieu de toute série d'observations qui dépasserait l'heure de minuit.

Le Prof. Safarik, de Prague, insiste sur l'idée de sacrifice précitée. Il constate que ce sacrifice ainsi fait au public, n'est nullement exigé par lui : pourquoi dès lors se l'imposer ?

Le Prof. Krüger, de Kiel, pense qu'il existe fort peu de points communs entre l'astronome et le public. Là où il en existe, un compromis pourrait aisément s'établir sans troubler une habitude prise d'ancienne date, et qui a ses raisons d'être.

Le Dr. Dunér, de Lund, opine dans le même sens.

M. le Président Auwers regarde la question comme du ressort intime de l'astronomie. L'habitude admise pourrait être discutée s'il s'agissait de l'établir, mais elle existe, et toute discontinuité introduite dans la manière de compter le temps serait la source de graves désagréments.

Après lui M. H. G. v. d. Sande Bakhuyzen, de Leide, se voit retirer la parole au moment où il veut arguer, en faveur du maintien de la coutume admise, du désir des marins de conserver le compte du jour à dater de midi, appuyant ainsi le point de vue des astronomes : il a été entendu au début que les opinions de ces derniers seuls seraient invoquées.

Le Prof. Gyldén, de Stockholm, en présence d'un désir général de l'ensemble des astronomes d'adopter le changement, se rangerait à la résolution malgré ses inconvénients réels. Mais comme nous sommes encore loin de cette condition réalisée, il n'y faut pas songer.

Le Prof. Tietjen, de Berlin, représente dans l'assemblée la cause du 'Jahrbuch,' dont le rôle est important dans la science, et dont l'opposition a été précédemment manifestée. Il déclare toutefois que cette opposition cessera lorsque la majorité des astronomes le demandera. Ce ne sera en tout cas pas avant l'an 1900, pour ne pas dire l'an 2000.

Le Dr. Pechüle, de Copenhague, est moins effrayé que plusieurs de ses collègues par l'introduction du nouveau mode ; mais il est aussi d'avis que pour y songer, il faut que l'unanimité des astronomes lui soit acquise.

Enfin le Prof. Folie, Directeur le l'Observatoire de Bruxelles, après s'être montré disposé à se prêter au plus tôt à une tentative d'assimiler le jour astronomique au jour civil, reconnaît, sur l'observation du Président, que des mesures isolées, prises dans ce sens, entraîneraient de graves inconvénients. Il termine en réclamant, comme un progrès à réaliser sans délai, l'adhésion de tous les savans, de toutes les institutions à la réforme, déjà antique aujourd'hui, du calendrier Grégorien.

Le compte rendu qui précède reproduit en abrégé, avec toute l'exactitude possible, l'échange d'idées qui s'est produit pendant la séance du congrès. S'il a offert un ensemble d'opinions très différent de celui qui s'est manifesté pendant la délibération du 20 Octobre 1884, à Washington, cela tient probablement à la composition purement astronomique de l'assistance.

Nous savons qu'un astronome véritable, et parmi les plus éminents, délégué de la Grande Bretagne, de l'autre côté de l'Atlantique, a montré une complaisance surprenante en faveur de l'innovation proposée, et qu'il a par là même puissamment contribué à l'adoption du vœu formulé dans la résolution. La réunion de Genève lui aura prouvé que ses confrères de l'ancien monde ne sont pas disposés à faire bon marché de leurs traditions et de leurs habitudes. De puissantes raisons ont été données pour les conserver, raisons vaillamment soutenues aussi par le plus autorisé de tous les astronomes Américains.

L'introduction du jour universel venait d'être votée à la conférence de Washington, malgré une très grave modification aux propositions antérieures, avec la sage réserve contenue dans le texte de la IV^e résolution, ainsi conçue :—“ La conférence propose l'adoption d'une heure universelle pour tous les besoins pour lesquels elle peut être trouvée convenable. *Cette heure ne devra pas empêcher l'usage de l'heure locale, ou d'une autre heure normale qui paraîtra désirable.*”

Cette dernière clause paraissait destinée à rassurer les simples mortels sur l'emploi de leur ancienne routine pour compter le temps, en usant du midi et du minuit de leur horloge, et à rassurer les astronomes et les marins, libres de continuer à suivre leur mode spécial de commencer leur jour. Comment le vœu émis dans la VI^e résolution vient-il détruire ces assurances tranquillisantes, exciter des opposition capables de nuire à l'un des buts principaux de la conférence ? Nous ne saurions l'expliquer.

On ne peut se le dissimuler : il y a encore beaucoup à faire pour amener l'adoption de la mesure, même pour ceux qui y ont un intérêt direct. Les savans qui leur ont fourni l'idée de l'heure universelle en vue des télégraphes, des voyages de long cours, des grandes lignes des chemins de fer, devront-ils être contraints de changer leurs coutumes anciennes et respectables lorsqu'ils y trouvent de sérieux inconvénients ? et cela avant que nul parmi les premiers interessés ait pris des mesures efficaces pour l'appliquer ?

Dans le cas possible, où le système proposé viendrait à échouer, avant même d'être essayé, quel rôle de dupes n'auraient pas joué ceux qui précipitamment modifieraient leurs procédés.

Le jour universel a été clairement défini à Rome en 1883, mais profondément altéré à Washington au point de vue des astronomes; ses conditions y ont été déterminées, justifiées, discutées; mais une étude consciencieuse et de longue haleine est nécessaire pour le rendre utilisable. Cette étude est du ressort des grandes administrations. Jusqu'ici rien n'a transpiré en ce qui concerne sa poursuite; on peut même suspecter un peu le zèle qu'elles y apportent. Dans ces circonstances, pourquoi venir parler changements aux hommes de science, dont les agissements sont sans influence aucune sur la grande mesure dont il est question de doter l'humanité. Que les astronomes comptent leur jour à partir de midi ou de minuit, ils sauront toujours fournir le temps moyen à ceux qui le désireront, quel que soit le méridien pour lequel on viendra le leur demander.

Un auteur dont la compétence est grande dans la matière, le Prof. Th. von Oppolzer, de Vienne, est parti d'un point de vue fort différent pour préconiser l'innovation, même dans la science qu'il illustre. Il prévoit la réforme adoptée par l'univers entier, et suppose, avec Mr. Sandford Fleming, le public des cinq parties du monde acquis à la nouvelle manière de compter le temps, l'heure universelle employée sous tous les méridiens.

Pour cette époque, qui nous paraît encore un peu du ressort de l'utopie, il prévoit, en homme pratique, les procédés les plus commodes pour lever les obstacles dont M. Newcomb et ses collègues, calculateurs d'éphémérides, sont aujourd'hui frappés.

Dans cette hypothèse, nous ne doutons pas qu'avec le concours de tous les savans, dont la résistance ira s'amoindrissant, on n'arrive à une entente donnant satisfaction à tous. Mais, pour le moment, nous concluons avec la quasi unanimité des astronomes réunis à Genève au mois d'Août 1885: prudence, pas de précipitation intempestive; conservons notre indépendance, qui n'est nuisible à personne; voyons venir le progrès, s'il s'établit; mais ne nous laissons pas entraîner dans une voie ingrate, qui a ses dangers, pour l'unique avantage de faire du nouveau.

Genève, 1885, Octobre 21. E. Gautier.

Remarks on Dr. Wilsing's Experimental Examination of the Wedge Photometer.

In the last issue of the 'Observatory' there was given a condensed summary of Dr. Wilsing's experimental examination of the Wedge Photometer. Dr. Wilsing's remarks are, as might be expected from him, discriminating as well as able and exhaustive. As the work criticised by him is that contained chiefly in Vol. xlvii. of the 'Memoirs of the Roy. Astr. Soc.,' it does not refer to a very large

number of the fainter stars, my labours regarding which will be published by the Delegates of the Clarendon Press, under the title of 'Uranometria Nova Oxoniensis,' probably by the time this article is in the hands of your readers.

Dr. Wilsing, as the result of his experimental and exhaustive enquiry, comes to the conclusion that, provided certain evident and ordinary precautions are taken, the Wedge Photometer is "a valuable addition to our instrumental aids." With this opinion from an authority so competent, astronomers may well rest entirely satisfied, since I did not hope to devise an instrument which would produce valuable results if incautiously and unskilfully used, but one which should neither present any unusual difficulty, nor possess any fundamental defect, if fairly handled.

There are, however, a few remarks connected with Dr. Wilsing's researches, on the four points stated in your issue, which, as being due to that eminent astronomer and as probably interesting to your reader, I propose to make *seriatim*. As to the homogeneity, selective absorption, and geometrical figure of the wedges, I am glad, but not surprised, that my able critic considers the enquiry thoroughly satisfactory: these are points which I consider as of far more practical importance than any minute physiological considerations under which the instrument is examined.

I. Dr. Wilsing "thinks that the use of the method of limiting apertures for the determination of the wedge constant is to be avoided, because the brightness in individual *objectives* is not directly proportional to the aperture." In this I am absolutely at one with Dr. Wilsing. I abandoned the method of apertures in regard to the wedge for the very reasons assigned by him; nevertheless my own numerous experiments show that I might have adopted the method without introducing that serious amount of error which he derives from some object-glasses of a different character to those used at Oxford. Wolf and Ceraski have met with the same sort of results herein as Dr. Wilsing; but Dr. Müller operating with an object-glass of 135 mm. does not find the results so objectionable *. Nevertheless I, with Dr. Wilsing, distrust the method. These remarks do not apply to Mr. Knobel's use of an unsilvered mirror.

II. "That it is not right to assume the invariability of the eye for any length of time." In the abstract this remark must be tenable; but in the mode in which the instrument is used at Oxford it is shown, by reference to a table containing the observations of a single night, that there is evidence of the constancy of the observer's eyes during the observations of Polaris taken at the commencement, the middle, and the end of the extinctions. Even if the sensitiveness of the eye were practically variable, the division of the observations in this way would secure a result in accordance with its *mean* condition. Even Dr. Wilsing's own example of work

* Publicationen des astrophysikalischen Observatoriums zu Potsdam, No. 12, p. 11.

during the long period of five consecutive hours, points, I think, as much to some accidental and lawless circumstances as to the gradual degradation of optical sensitiveness.

III. "That disturbing influences make themselves felt, particularly in the fainter stars, when the brightness of the ground forms an important fractional part of the total intensity." When Dr. Wilsing made this important remark, the Oxford work on the very large number of stars of the fifth and sixth magnitude was not published. I have carefully examined these stars, taken in groups situated in various portions of the sky of variable stellar density, and on comparing them with the "Harvard Photometry" I find no trace of systematic error from this cause. This, however, assumes the "H. P." to be itself free from the same error. But, from a theoretical point of view, such error cannot exist in the Wedge Photometry, owing to the extreme smallness of the field (a diameter of 80″), which will very seldom contain, together with the star examined, stars of a magnitude sufficient to effect the determination. This will be apparent on examining the stars in Argelander's charts. Suppose in this minute field two stars existed discreetly, of the eighth and tenth magnitude, then the eighth magnitude star could be accurately compared with Polaris, the tenth magnitude being extinguished long before the eighth, and would not affect the determination of the brighter star. The tenth, however, would not be accurately measurable in the presence of the eighth, except by a double-image photometer. But such cases are *extremely rare* and do not at all touch such stars as I have examined photometrically. For any such stars as I am concerned with up to the present time, ordinary moonlight and at moderate distances from the Moon has no appreciable effect. It is extinguished long before the star. I may also remind observers that sixth magnitude stars are distinctly visible in their occultation at the bright limb of the Moon.

IV. "That comparisons of different coloured stars are in general not comparable immediately with direct estimations." Regarding the question of colour, I can only repeat what has been so strongly insisted on in Vol. xlvii. of the 'Memoirs,' viz., that, so far as colours not very salient are concerned, the wedge interval for them is identical with that for light in general. At the same time I cannot repress the conviction that the photometry of intense colours is not as yet accomplished, and may be connected with the domain of physiology.

I have made good progress towards the re-measurement, with many extinctions, and on several nights, of an equatoreal zone of about 50 stars, comprising fourth and sixth magnitudes, whereby the constant appeal to Polaris may be rendered unnecessary, and at the same time means will be afforded of determining the scale of a wedge, without having recourse to the method of apertures, or that ofdouble refraction.

Oxford University Observatory,
1885, Nov. 18.

C. Pritchard.

Kayser on the Spectroscope *.

THIS text-book of the spectroscope is so thoroughly good that we can warmly commend it to every spectroscopist who can read the German tongue. For the story of the growth and development of the new analysis, the student will still turn to the pages of Roscoe and of Schellen, rather than to the concise though very accurate summary of it here given; but for a conspectus of the actual results obtained, brought well up to date, we do not hesitate to say that the treatise now before us is a great advance upon any working manual of the spectroscope previously in existence.

The book is in three sections. The first, entitled "The Radiation of Light," gives a remarkably clear account of the laws of refraction and dispersion, and of the various processes and instruments whereby the analysis based upon those laws is conducted. The next section, "The Absorption of Light," is largely concerned with the solar spectrum, and with the manifold issues of Kirchhoff's law, whereof a very full mathematical demonstration is given. The third section is a catalogue of all the known chemical elements in alphabetical order, each article being headed by a chronological list of all pertinent papers of any importance known to exist in English, French, or German, in addition to a copious bibliography elsewhere.

Then we have a discussion of the observations of the element in question, followed by a table of all its known lines, so arranged in parallel columns as to show the determinations by several authorities, usually Huggins, Thalén, Kirchhoff, and Lecoq, with Cornu for the ultra violet, though very many other observers are quoted upon occasion. Next, where desirable, is an account of the band spectrum, also with tables in full, as likewise a notice of the principal compounds. At the end of the entire section is a very valuable table of wave-lengths for every known line in the visible spectrum; from Thalén's measures, supplemented where necessary from other sources. In all the tables the author has wisely chosen to merely indicate the chief lines of each element by bolder type, on the ground that the exponent figures depend so much on casual circumstances of pressure and of temperature, as to form no safe guide for identification. Throughout the book no pains have been spared to give the reader the benefit of the most recent researches, with especial attention to the investigations of Lockyer, and of Liveing and Dewar. Nine plates are appended, giving a very clear photographic reduction of Angström's atlas, together with Abney's and Cornu's maps brought to the same scale.

The only fault we have to find with the general balance of the book, otherwise singularly judicious, is in the extreme compression of the portion which relates to heavenly bodies other than the Sun. Here we will make free to retort upon the author the quaint reclamation in his own preface concerning the treatment of his subject in text-books of general physics, and complain that in like manner, this point is "ausserordentlich stiefmütterlich behandelt." We hope also in a new edition to see some mention of the successful application of the spectroscope to the detecting not merely movement of translation in the line of sight, but of axial rotation. The matter is of importance, since it is just possible that this is the direction in which we may look for the next great stride in our knowledge of the universe, when appliances shall have become so perfected as to admit of the assigning rotation periods and axis positions to certain of the bright line nebulæ, and so arriving at their cosmical relations.

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* 'Lehrbuch der Spektralanalyse,' von Dr. Heinrich Kayser. Berlin: Julius Springer, 1883. 8vo. Pp. 358.

The book is remarkably accurate. We observe a trifling erratum on p. 294, a manganese line being misplaced, λ 6521 for λ 6021; and it would perhaps be better in the table on p. 161, with regard to the double lines D and H, to follow the established usage, and reckon as D_1 and H_1 the lines least refrangible in each pair. The arrangement is excellent, the small sub-chapters with bold type headings greatly facilitating reference; and the paper, type, and abundant illustrations complete a most satisfactory volume.

We very sincerely congratulate Prof. Kayser on the well-earned promotion which has recently transferred him from a subordinate position in the University of Berlin to an appointment of much consideration at Hanover. S. H. Saxby.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Meteors with Fixed or Stationary Radiants.

Gentlemen,—

I venture to offer an explanation of many of the difficulties involved in meteors with fixed or stationary radiants which seems to me more satisfactory than any that I have hitherto seen.

A meteor entering our atmosphere is in the same condition as a ship under full sail or steam which suddenly enters a current. The former motion continues, but the current impresses a new motion on it, and the subsequent motion is the resultant of the two. To an observer floating down with the current, however, the result would be different. Insensible as to his own motion, he would see the ship in exactly the same position at every instant as if the current did not exist. Applying this principle to meteors which penetrate our atmosphere to such a depth as to become luminous (owing to its resistance, or rather perhaps to their collision with the moving particles of air) it would appear that so long as they reach us in the same direction they will seem to emanate from the same radiant-point, inasmuch as the meteors and the spectator equally participate in the motion of the Earth.

The same theory affords an explanation of the fact that the radiant-point of meteors appears to be the same throughout the night, although the rotation of the Earth ought theoretically to make it shift to an extent which can hardly be regarded as inappreciable. This difficulty is at once removed if we suppose that during the whole period of its visibility the meteor partakes in the rotatory as well as in the orbital movement of the atmosphere in which it is immersed.

The great tenuity of the air in its upper regions affords the principal objection to this explanation. But air which is sufficiently dense to heat the meteor to luminosity (up to which time it is not visible) must, I think, be dense enough to impress its own motion mechanically on the meteor. If so, the meteor already partakes in the Earth's motion at the moment when it first becomes visible.

On this theory of course the meteors which enter our atmosphere are diverted from their original orbit, and if not dissipated in the atmosphere will thenceforth follow a new track. This may perhaps be the origin of some of the attenuated meteor-streams noticed by Mr. Denning and others. Such attenuated streams might also arise from the passage of a large meteor-stream near some other planet; and the derivative stream generated in this way might pass near the Earth's orbit in cases where the original stream passed far outside it. And the connection of the original stream with some particular comet might be quite lost sight of, when we had only the derivative stream to examine.

If this view should prove to be correct, the Earth may evidently influence the future course of a comet which approaches near enough to us, otherwise than by means of gravitation. It is not impossible that this may have occurred in the case of Biela's comet thirteen years ago. The closing days of the present month will be a critical period in the history of that remarkable comet.

Yours truly,
W. H. S. MONCK.

Dublin, 1885, Nov. 7.

Halley's Southern Catalogue, reduced by Sharp.

GENTLEMEN,—

Perhaps you will allow me to call attention to the extraordinary way in which Sharp's Catalogue of southern stars, reduced from a part of Halley's observations at St. Helena, is given in the 'British Catalogue of Stars' (vol. iii. p. 77), as published in 1725. No mention whatever is made of Halley, and one cannot help suspecting that his name was left out by Flamsteed's orders by design. The stars selected are those which are not visible at Greenwich, so as to make the Catalogue strictly supplementary to that of Flamsteed; these are 265 in number, whilst the number actually observed by Halley was 341. The heading in the 'Historia Cœlestis' is as follows:—

"Abrahamus Sharpius, *De villa vulgo vocata* Little Horton, *prope Urbem de* Bradford *in Agro Eboracensi*, Μαθεσεως *Vir per-*

peritus ; postquam antecedentem
MSTEEDIANUM *ad Annum* 1726 *provectum*
a Fixarum Distantiis *quibusdam in Insula* Sanctæ *Hel*
atis, sequentem Fixarum *quarundam* Australium *in Nostro*
ispherio non adspectabilium Catalogum *Nobis exaravit et ad*
um 1726 *adaptavit.*"

n the first four pages, the column which contains the south polar lance is headed "Distantia a Polo B," instead of "Distantia a olo A."

All due credit should of course be given to Sharp for the great bour of reducing the observations from the form in which the esults were left by Halley; but the name of the observer ought certainly not to have been omitted.

Whilst speaking of Sharp, I may, perhaps, mention that the late Mr. John James, F.S.A. ('History and Topography of Bradford'), traced the nature of his relationship to the Rev. John Sharp, who became Archbishop of York in 1691. The fact of some consanguinity is stated in Sharp's epitaph in Bradford Church, but Whitaker, in his 'Loidis and Elmete,' regrets that Thoresby failed to ascertain what its nature was. Tracing the pedigree a generation further back than Thoresby had done, Mr. James found that Abraham Sharp and the Archbishop were in fact second cousins, their respective fathers (John and Thomas Sharp) having been first cousins to each other.

I need hardly mention that Abraham Sharp was born at Little Horton in 1651, and was first brought into the Royal Observatory by Flamsteed in 1688 as his amanuensis in succession to J. Stafford, who had died in May that year. His mathematical and mechanical skill, as well as his love of calculation, made him of great use to the Astronomer Royal, particularly in the construction of his great mural arc. After he left the Observatory, on account of his health, he continued to correspond with Flamsteed as long as the latter lived; and he made observations at Little Horton with instruments constructed by himself. He died there on the 18th of July, 1742, in the ninety-first year of his age.

Yours faithfully,
W. T. LYNN.

Blackheath, 1885, Sept. 5.

P.S.—Whitaker, in quoting the epitaph on Sharp in Bradford Church to which reference is made above, has the following quaint remark upon its comparative brevity and omission to mention what that writer appears to have considered Sharp's principal achievement:—

"Ludolph Van Ceulen, a Dutchman, computed the quadrature of the circle to 136 places of decimals, and had the process inscribed upon his tomb. Our countryman far (but I forget how far) surpassed him, yet has a much shorter epitaph."

Van Ceulen (in imitation, it is to be presumed, of Archimede with regard to the proportion which the content of a sphere bea to that of its circumscribing cylinder) directed that the approximation which he had calculated between the diameter and circumference of a circle should be inscribed on his tomb. But Whitaker greatly exaggerates the number of figures to which he carried this approximation, which was not 136, but only 36 decimal places. Sharp extended it (see Sherwin's 'Mathematical Tables') to 72 decimals; so that if Whitaker's argument be just, he ought to have had an epitaph exactly twice as long as Van Ceulen's. Machin (who succeeded Dr. Torriano as Professor of Astronomy at Gresham College in 1713 and died in 1751) was the first to carry the value of π in the circle to 100 decimal figures.

Cornelius Gemma and the Star of 1572.

GENTLEMEN,—

As I find it stated in some books on astronomy that the first discoverer of the famous star of 1572 (which was so successfully observed by Tycho Brahé) was Gemma Frisius, it may be well to point out that writers who do so confound him with his son, Cornelius Gemma. The father—called Frisius, from having been a native of Dockum, near Leeuwarden, in Friesland—died in 1555, more than seventeen years before the appearance of the star in question. He became Professor of Medecine at the University of Louvain, and wrote, as is well known, several works on astronomy, geometry, and cosmography. His only son, Cornelius Gemma, was born at Louvain in 1535, and died of the plague in 1579, after holding for some years the same professorship as his father had done before him. He wrote a small Latin treatise on the star which he appears to have been the first to notice. In Mr. Knobel's "Reference Catalogue of Astronomical Papers and Researches," in the Supplementary Notice for 1876, forming No. 9 of Vol. xxxvi. of the 'Monthly Notices,' this tract is relegated to the Appendix, whence I conclude that he was unable to find it*. Though not in the Library of the R.A.S., it is, however, in that of the British Museum, and is before me at this moment. Perhaps it will be interesting to quote in full the first sentence, by which it appears that Cornelius Gemma noticed the star on the evening of Sunday the 9th of November (two days therefore before it was seen by Tycho Brahé), and that he felt sure it was not visible on the preceding day (the 8th). He writes:—

"Novus hic phosphorus (liceat verò sic propter apparentem similitudinem appellare) cœpit fulgere primum anno hoc à Christo nato 1572, nona Novembris, die dominico vesperi, cùm tamen observantibus proximum cœli locum die octavo, etiam sereno æthere non apparuerit."

The tract is bound up with another, containing reflections on the phenomenon by Postellus, and the titlepage is common to both; the date appears to be 1573, and the title is "De Peregrina Stella quæ superiore anno primum apparere cœpit." This tract of Postellus is not included in the list of his works in Zedler's 'Universal Lexicon.' In it he speaks of Cornelius Gemma as "Frisius;" but that cognomen, properly applicable to his father, is not so to himself.

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* Mr. Knobel, however, includes in the Appendix not only works of that kind, but others which had unintentionally escaped notice in the Catalogue itself.

Yours faithfully,
W. T. Lynn.

Blackheath, 1885, Oct. 13.

The Red Star Schjellerup 238.

Gentlemen,—

With reference to the announcement in the 'Observatory' for November, that Prof. Safarik had discovered variation in this red star, which is identical with No. 545 of Birmingham's Catalogue, I beg to point out that its variability was discovered by Secchi in 1869 (p. 323 of Birmingham's Catalogue) and detected (independently) by myself in 1875 in Northern India (p. 107 'Southern Stellar Objects'). In accordance with Argelander's well-known nomenclature, I proposed at the time to designate it V Capricorni. The star is No. 619 of my Catalogue of Suspected Variable Stars, where further details will be found. Some of the continental astronomers seem curiously ignorant of the work done by other observers. Early in the present year a well-known French astronomer announced his discovery of R Leonis as a *new* variable star! Perhaps Mira Ceti next!

Yours faithfully,
J. E. Gore.

The Lick and Washburn Observatories.

Gentlemen,—

The Lick Observatory is a gift from Mr. James Lick to the State of California. It is to be built and completely equipped under the direction of the board of Lick Trustees, of which Captain Richard S. Floyd is President. The Observatory, when completed, and the unexpended balance of the original fund of $700,000 are then to be turned over to the Regents of the State University of California, as the Astronomical Department of that University. I have been elected President of the University of California and Director of the Lick Observatory, and I shall assume these new duties about Jan. 1, 1886.

May I ask, through your columns, that letters and parcels destined for me personally should be directed to me at Berkeley, California (the seat of the University), while parcels for the Observatory should be addressed—"Library of the Lick Observatory, San José, Santa Clara County, California."

I desire to thank the many astronomers and societies who have generously added to the library of the Washburn Observatory by their gifts, and to ask of them a similar generosity for the Lick Observatory, where a large library is forming.

Real astronomical work at the Lick Observatory will begin as soon as it is possible. Under provisions of the Trust, no salaries can be paid to observers until after the completion of the Observatory, and this date depends upon the time at which the large objective is finished by the firm of A. Clark and Sons. Everything else is practically complete.

I have hopes that some arrangements may be made by which the meridian circle and the 12-inch Clark refractor may soon be put to use. The Washburn Observatory will shortly print its volume iv., containing so many of the observations of the 303 fundamental stars as I have been able to make since May 1884, when this work was begun.

Yours faithfully,
EDWARD S. HOLDEN.

Washburn Observatory,
University of Wisconsin, Madison.
1885, October 20.

Hyginus N.

GENTLEMEN,—

Almost immediately after the announcement of Dr. Klein's discovery of Hyginus N, Mr. Wesley and myself, and later several other selenographers and myself, carefully examined the very copy of the first edition of Mädler's map of the Moon referred to by Mr. Rand Capron in your number for September.

The marking there shown is neither of the form, position, size, nor nature of Hyginus N, but is the well-known full-moon marking beyond it.

By reference to a better preserved copy of this first edition of Mädler's map, this will be clearly seen. His special drawing and map of this region renders it unquestionable.

I may add that the whole matter was thoroughly examined and settled years ago.

Yours faithfully,
E. NEISON.

NOTES.

STELLAR PHOTOMETRY.—In an exhaustive account (V. J. S. 20 Jahrgang, S. 193–209) of Herr Wolff's photometric observations* Dr. G. Müller of Potsdam points out the remarkable discordances which the comparison of Wolff's results with those of Argelander's 'Uranometria,' and with the Photrometric Catalogue of C. S. Peirce, contained in Volume ix. of the 'Annals of the Harvard College Observatory,' has brought to light. Herr Wolff's observations were made with a Zöllner photometer attached to a telescope of 37 millim. aperture, and form a continuation of his previously published volume 'Photometrische Beobachtungen an Fixsternen' (Breitkopf u. Härtel in Leipzig, 1877). The observations were made, for the most part, from 1876 to 1880, and the work contains measures of 972 stars, extending down to the sixth magnitude. The comparison with Argelander, to which Herr Müller refers, depends on 852 stars; and the following table exhibits the logarithm of the light (h) as determined by Wolff, corresponding to successive half-magnitudes as given by Argelander:—

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* Photometrische Beobachtungen an Fixsternen aus den Jahren 1876 bis 1883. Von Julius Theodore Wolff. Georg Reimer: Berlin, 1884.

Mag.	Log h.	Diff.	Mag.	Log h.	Diff.
2·0	9·3296	−0·1815	4·0	8·6330	−0·1269
2·5	1·1481	0·1868	4·5	8·5061	0·1033
3·0	8·9613	0·1752	5·0	8·4028	0·0888
3·5	8·7861	0·1531	5·5	8·3140	0·0893
4·0	8·6330		6·0	8·2247	

It will be seen that the difference of log h for a star of the second and one of the third magnitude is 0·368, whilst the corresponding quantity found in passing from the fifth to the sixth magnitude is only 0·178. The mean difference corresponding to one stellar magnitude is 0·28, whilst the value of this quantity according to other authorities varies from 0·34 to 0·46. It is evident therefore that Wolff makes either the brighter stars too faint, or the fainter stars too bright; and Herr Müller concludes, from the evidence before him, that the latter alternative is the more probable one. The comparison with Peirce's results, also obtained with a Zöllner photometer, is thus expressed: $\frac{W - W_0}{P - P_0} = 0{\cdot}7382$—that is, the difference of the logarithm of the light of two stars as determined by Wolff is in a constant ratio to, and always less than, the same difference as found by Peirce. Should the latter measure the difference of magnitude between two stars to be four stellar magnitudes, the former would find it to be but three stellar magnitudes. If, in addition to these facts, it be noted that in his previously published work (referred to above) Herr Wolff, from a comparison of his measures with Seidel's, arrived at very similar results, and also that Prof. Pickering's recent photometric observations show discordances agreeing in sign and sensibly of the same magnitude, we may conclude, in Herr Müller's opinion, that the observations of Wolff differ from those of the great photometric catalogues in the sense that he makes the fainter stars too bright. Herr Müller is unable satisfactorily to account for this peculiarity in Wolff's measures, as Peirce, Lindemann, and he himself (all three using Zöllner photometers) obtain no such discordant results. It may arise from difference in the sensibility of the eye or in the method of using the photometer; though Herr Müller is careful to explain that he thinks Wolff's arrangement of the observations and methods of working are such that it is improbable that systematic errors having their origin in the method of using the instrument should exist, and considers that the point deserves a further thorough investigation.

Herr Lindemann, of Pulkowa, has recently published * an interesting memoir containing photometric observations of the Pleiades stars, entitled "Helligkeitsmessungen des Bessel'schen Pleiadensterne." Using a Zöllner photometer and a 5-inch telescope, it was possible to measure 52 out of the 53 stars observed by Bessel, the remaining one, Anonyma 16 (9·10 mag.), being too faint for the instrument. Each star was observed on two separate nights. The star magnitudes depend on the assumed magnitude (4·22) of Merope. This magnitude of Merope has also been assumed both by Prof. Pickering and by Prof. Pritchard in their photometric observations of the Pleiades group. In addition to Merope, Herr Lindemann has used as reference stars Celæno and Anon. 32, the magnitudes of which he has found from his measures to be 5·27 and 6·51 respectively. In transforming the logarithm of the light into the corresponding stellar magnitude, Pogson's scale has been used. The observations extend from September 1883 to March 1884.

The author has compared his results with those of Bessel, Argelander, C. Wolf, Pickering, and Pritchard. The agreement with Pickering and Pritchard is, on the whole, very satisfactory, the mean discordance in the former case being 0·04, and in the latter case (using Pritchard's latest results) 0·01 of a magnitude. A few stars stand out with abnormal discordances, and Herr Lindemann thinks that he is justified in assuming that the following are variables, viz. Anon. Nos. 1, 4, 21, 31, and 33, and possibly also 18 *m* and 26 *s*. Considering that these three series of observations have been made with photometers differing so widely in principle and construction, the agreement here found must be considered to be very satisfactory, and appears to indicate that differential measures of stellar brightness (at all events within the limits of magnitude here considered) can now be made with very great accuracy. Herr Lindemann has made a valuable addition to our knowledge of an interesting and important branch of astronomy.

The same astronomer has also published † a determination of the variation of the light of the red star V Cygni. Using five comparison stars, ranging in magnitude from 6·5 to 10·2 according to his measures (the magnitude of one of the comparison stars, viz. 8·5, being assumed from the DM.), Herr Lindemann finds the following times of maximum brightness for V Cygni:—1882 Aug. 31, Mag.=6·8; and 1883 Aug. 5, Mag.=7·3. At minimum the star falls below the tenth magnitude, and is then too faint for the telescope with which these observations were made. The variable was observed on one evening in 1881, seven evenings in 1882, and twenty-two evenings in 1883. The colorimeter of the Zöllner photometer was set to two different readings during the observations, the artificial star being made white when the variable was faint, and deep red when it was near its maximum. Herr Lindemann points out that Schmidt found the time of maximum of V Cygni in 1882 to be July 17, whilst Safarik found it to be June 17, and being unable satisfactorily to account for this discordance, concludes that as yet he can only say that the period is about one year.

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* Mémoires de l'Académie Impériale des Sciences de St. Pétersbourg, 7^e sér. Tome xxxii. No. 6.

† Mélanges Math. et Ast. tirés du Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, Tome vi. pp. 215–230.

With regard to the general question as to the advantage of using photometers or of depending on eye observations for determination of stellar brightness, we may quote the opinion of Mr. S. C. Chandler, Junr., as recently expressed in these words *:—"In the investigation of the variable stars, Argelander's method of observation has proved, in precision, convenience, and fruitfulness, superior to any photometric apparatus yet devised."

THE LICK OBSERVATORY.—We have great pleasure in publishing Prof. Holden's letter announcing his appointment to the Presidency of the University of California and the Directorship of the Lick Observatory. We heartily congratulate Prof. Holden on his appointment, and not less so the Lick Trustees on having secured the services of so able and energetic an astronomer. It is especially fortunate that the regents of the university have also agreed to select Prof. Holden as their President.

Prof. Holden has strongly recommended Prof. Rogers, of Harvard College, as his successor at the Washburn Observatory.

THIRD REPORT OF THE COMMITTEE ON STANDARDS OF STELLAR MAGNITUDES.—The third report of this Committee has recently been published. The scrutiny of the regions from which the standards are selected has been carried on. The maps of these regions were originally made with the 15-inch telescope of the Harvard College Observatory; and they have been carefully revised with the slightly larger telescope of the Washburn Observatory, and again with the Princeton refractor of 23 inches aperture, and a last revision with the great Washington refractor of 26 inches will probably follow. From these twenty-four maps stars suitable as standards have been selected, and their magnitudes determined, either with Photometer I. of the Harvard Observatory, or with the large meridian-photometer or with a wedge-photometer. The present report contains the results of these observations. A second table contains the magnitudes of 21 close circumpolar stars.

THE GREAT PULKOWA REFRACTOR.—Dr. O. Struve has written to Messrs. Alvan Clark and Sons acknowledging the excellent performance of the great object-glass which they furnished, and announcing that the Emperor of Russia has conferred upon them the golden honorary medal of the Empire. Messrs. Clark and Repsolds are the first to receive this token of honour from the present Emperor, which is only bestowed for quite extraordinary merits.

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* Science, Vol. vi. No. 137, p. 248.

THE ROTATION PERIOD OF MARS.—Prof. Bakhuyzen has recently published a very painstaking memoir on the rotation period of Mars, in which, after a thorough discussion of a very large number of observations extending from those of Huygens in 1659 to Schiaparelli's in 1879, he obtains a value which may be accepted with confidence as a distinct improvement even on the accurate determinations which we already possess. His method has been to assume Proctor's value for the rotation period, and Schiaparelli's place for the north pole of Mars, and to deduce the areographic longitudes of the principal markings on the planet with these elements; afterwards he obtains corrections to the assumed elements from a comparison of these computed longitudes. The value he thus obtains is $24^{h}\ 37^{m}\ 22^{s}\cdot66 \pm 0^{s}\cdot0132$, which accords very closely with Kaiser's, viz. $24^{h}\ 37^{m}\ 22^{s}\cdot62$, probably the best we have had hitherto. Proctor's value is too large, as the following table of the mean longitude of the northern point of the "Kaiser Sea," computed with it for different oppositions, will show:—

		°	Weight.
1661.	Huygens	315·7	1·5
1666.	Hooke	296·4	0·3
1782.	Herschel	305·8	3
1799.	Schroeter	303·3	16·5
1830.	Beer and Mädler	299·5	3
1862.	Kaiser, Lockyer, and Lord Rosse	294·9	7·5
1864.	Kaiser and Dawes	294·3	3
1877.	Schiaparelli, Lohse, Green, Niesten, and Dreyer	289·6	15
1879.	Schiaparelli	288·4	8

There is a steady decrease in the longitude as time goes on, the only observations which stand out seriously being those of Hooke; and as it was on these two drawings of Hooke that Proctor principally relied, the excess of his determination is easily explained. It is not possible either to make Huygens' observations agree very closely with the observations of the present century, but his sketches were always exceedingly rough. On the whole, however, this investigation shows that a very considerable amount of reliance may be placed on the accuracy of the principal drawings of Mars which we possess.

Prof. Bakhuyzen adds a note on changes on the surface of Mars, from which it appears that "Huggins Inlet," Schiaparelli's "Cyclopum," was a much more conspicuous object in the days of Schroeter and Sir William Herschel than it has been of recent years and that it was then comparable in size, shape, and definiteness with the "Kaiser Sea." Schroeter appears also to have observed the "canal" "Læstrygonum," which he could scarcely have done had it not then been much more conspicuous than it was in the late oppositions.

A table of the times of transit of Prof. Bakhuyzen's adopted prime meridian over that meridian of Mars which passes through the north pole of the Earth, for any date in the present century, concludes the memoir. The prime meridian adopted nearly coincides with Beer and Mädler's spot *a*, the well-known "Dawes Forked Bay." With this prime meridian, the longitude of *d*, the "Terby Sea," is 90°·87, and of *f*, the "Kaiser Sea," 282°·00.

HONGKONG OBSERVATORY.—Dr. Doberck, though but so recently installed as Director of this Observatory, has already issued his first annual volume of observations, an achievement which reflects the highest credit on his energy and skill. The bulk of the volume is occupied by the monthly and yearly weather reports for 1884. Thirteen appendices follow—mostly nautical circulars on typhoons and storms of the China seas of a most useful and practical character.

DOOLITTLE'S 'PRACTICAL ASTRONOMY'*.—This work is, the author states, designed as a manual for the field astronomer, and is admirably suited for such a purpose, being written with that elaborate attention to practical details which is a characteristic of books of this description coming from the United States. First, we have an Introduction to the method of Least Squares, not differing in fundamentals from similar chapters in Watson, Chauvenet, or Wright; but, if possible, going still more into detail on practical points than these authors have done, and explaining to the reader many of the "dodges" of computing, the best method of arranging the work, the various checks on the numerical accuracy of their computation most in avour with those who use the method of Least Squares, &c. The body of the work contains accurate and ample details as to the construction and use of the Sextant, Transit Instrument (in the meridian and in the prime vertical), the Zenith Telescope, and the Theodolite. The application of these instruments to the solution of the various problems for which they are suited is carefully explained and illustrated by numerical examples taken from actual cases which have arisen during work on the Coast and other Government Surveys. Special attention is paid to the determination of longitude. The various methods—transportation of chronometers, the electric telegraph, lunar distances, moon culminations, occultations of stars—are fully entered into; the mode of making the necessary observations and the subsequent reductions is carefully explained. In short Prof. Doolittle has spared no pains to make his book a practical one, not seeking after originality, but attempting to present in a systematic form the most approved methods in actual use at the present time. Chauvenet's still remains the most complete work on practical astronomy in the language, but in its more restricted field the book before us is calculated to be eminently useful. At the end of the volume are placed some Tables intended to facilitate the processes which are treated of in it. We have noticed the following misprints:—page 558, line 4 from bottom for *rest* read *rise*; page 587, line 1, for $2\mu\mu$ read $2\mu\mu'$; page 609, line 9, for *Nyréne* read *Nyrén.*

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* 'A Treatise on Practical Astronomy, as applied to Geodesy and Navigation.' By C. L. Doolittle, Professor of Mathematics and Astronomy, Lehigh University. New York: John Wiley and Sons. 1885.

THE LATITUDE OF THE BORDEAUX OBSERVATORY[*].—M. Rayet has determined the latitude of his observatory by measuring the zenith distances of a series of fundamental stars situated between the pole and the equator. These observations have always been made with the meridian instrument in both positions, and the determination of each evening depends on two or three observations of the nadir and on the observed positions of from 18 to 20 stars. According to the system of weighting adopted, M. Rayet finds for the value of the latitude $44^\circ 50' 7''\cdot 19$ or $44^\circ 50' 7''\cdot 27$, and the mean of these, viz. $44^\circ 50' 7''\cdot 23$, he regards a very exact determination of the latitude of the meridian circle. The longitude of the Observatory has from observations in 1881 been found to be $11^{m}\ 26^{s}\cdot 444$ west of Paris.

M. Rayet remarks that the values of the latitude obtained in the different months show a variation analogous to that found by Villarceau for the value of the latitude of Paris given by the pole star.

APPARENT ENLARGEMENT OF CELESTIAL OBJECTS NEAR THE HORIZON.—It will be remembered that in No. 96 (April) we gave the results of some experiments on this subject by M. Paul Stroobant. From 'Ciel et Terre' we gather that he has been pursuing further his researches. Suppose G is the size of an object at a certain altitude H, then the formula

$$G = 100 - 19 \sin . H$$

represents very well the relation between G and H if we regard 100 as the size on the horizon.

COMMENSURABILITY OF MOTIONS[†].—With regard to Prof. Hall's remark ('Observatory,' No. 102, p. 327) that "gaps in the ring of asteroids and divisions in the ring of Saturn have been explained on the theory that it is impossible for bodies to move in such places, since their motion would be commensurable with those of certain planets or satellites," Prof. Kirkwood points out that he never considered the question of stability, in the case of Saturn's ring, independently of collisions. He has shown that the matter which originally moved where the intervals are now observed was liable to extraordinary perturbation; and that, as the rings have considerable density, the disturbed portion would be brought into collision with either the exterior or interior parts of the ring, thus eliminating the primitive matter from the belts of greatest disturbance. In the case of the asteroids, their small comparative density would render collisions relatively less frequent. But the disturbed orbits of the minor planets would attain considerable eccentricity, so that the matter moving in them would, in perihelion, be brought in contact with the equatoreal parts of the solar mass, and thus become reunited with it. The origin of gaps is therefore not referred to the commensurability of periods as the simple or only cause.

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[*] Comptes Rendus, Tome ci. No. 16.

[†] 'Sidereal Messenger,' Vol. 4, No. 9, pp. 257–259.

THE BIELA METEOR-SHOWER.—The great uncertainty which envelopes the fate of Biela's Comet caused astronomers to look forward with especial interest to the probability that we might again, on Nov. 27 of this year, encounter part of that meteor-stream which is connected with the comet, and which yielded so rich a display on Nov. 27, 1872. As the comet's period, and presumably that of the meteors, is about 6·6 years, not quite two complete periods have elapsed since the earlier shower. It was very possible therefore, unless the meteors were pretty widely distributed along the comet's path, that we might escape an encounter with the stream on this occasion. The result has shown, however, that it is about as rich at the point where we intercepted it this year as where we met it in 1872. Many of the observations made in England were somewhat hindered by cloud, but abundant evidence was supplied of a very fine display. An observer at Keswick records that the "Bielites were uncountable." Another, at Leicester, says—"The display was most brilliant toward the western horizon, the meteors falling in perpetual showers, with brilliant trails like a very fine display of fireworks." At Greenwich the rate of fall between 6 and 7 o'clock was reckoned to be from 30 to 40 a minute. Prof. Pritchard, at Oxford, reports that they fell at the rate of 50 a minute from $6^h\ 34^m$ to $6^h\ 39^m$, and 60 a minute from $7^h\ 14^m$ to $7^h\ 19^m$; whilst Mr. G. Symons observed 5 in 1·6 seconds at $7^h\ 59^m$. This seems to have been the richest time of the shower, the frequency of fall diminishing notably a little later on, and at 11 o'clock the Greenwich observers were recording only 3 to 5 a minute. Observers further to the east, and favoured by cloudless skies, seem to have enjoyed a most superb spectacle, telegrams from Naples, Athens, Teheran, &c. speaking in the strongest terms of the beauty of the display. Dr. Copeland telegraphs from Dun Echt, under date Nov. 28, "Thousands of Biela meteors were seen here last night;" and Prof. Krueger, from Kiel, "Great meteor-shower, Radiant χ Andromedæ, was observed at Geneva. Please look for Comet Biela." Slightly different positions for the radiant were obtained by Mr. Denning and by the observers at Greenwich, the former giving it as R.A. 26°, Dec. 44° N., as for the 1872 shower, the latter from some 40 paths estimating it as R.A. 20°, Dec. 49° N.

A large number of the meteors were bright, being above the first magnitude, and a considerable proportion showed very short paths.

THE NEW STAR IN THE GREAT NEBULA IN ANDROMEDA.—M. Trouvelot, in the November number of 'L'Astronomie,' offers the following reasons for concluding that the star is not physically connected with the nebula. There are a multitude of small stars visible over the nebula, and these M. Trouvelot believes to belong to the Milky Way, of which he traces an extension beyond the nebula, since they increase in number as the Galaxy is approached. They are likewise all perfectly sharp and well defined, which they would not be if they were either in the nebula or behind it. The nebula he concludes therefore is behind this branch of the Milky Way. The well-defined appearance of the *Nova* and of a small star near it, which he believes to be also a new one, seems to prove that they are also in front of the nebula and to associate them with the Milky Way.

THE NICE OBSERVATORY.—The great object-glass for the Nice Observatory is completed and in the hands of M. Gauthier, who is constructing the equatoreal. The latter will be finished, it is expected, by the end of April. The glass was supplied by M. Feil, and worked by the Brothers Henry.

THE DEARBORN OBSERVATORY.—We have received Prof. Hough's report for the year ending May 20, 1885. The Repsold meridian circle has been used for determination of time, standard time having been furnished to the City of Chicago daily throughout the year. In connection with this instrument Prof. Hough has invented a printing Chronograph, a description of which we give on p. 442. The work with the great equatoreal has been confined to (1) Difficult double stars, (2) The Planet Jupiter, (3) Miscellaneous observations. The companion to Sirius has been measured on ten nights, with the results for 1885·197:—Position angle $=32^\circ{\cdot}7$, distance $=7''{\cdot}96$. Since 1880 the distance has diminished $2''$, the rate of decrease being now a little more than $0''{\cdot}5$ annually. The planet Jupiter has been systematically observed in relation to the physical phenomena seen on his surface. As in former years, the principal object of interest was the great red spot. Prof. Hough has already communicated to us the more interesting of the results which have been obtained by him with regard to this object ('Observatory,' No. 100, pp. 275–277). Four sketches of Jupiter exhibiting the salient features seen on his disk accompany the Report. The partial eclipse of the Sun of March 16 was observed for last contact. During the progress of the eclipse Mr. S. W. Burnham, Dr. H. D. Garrison, and Prof. Hough made a number of dry-plate negatives of the phenomenon. The most successful was that taken through red glass. No mention is made in the Report of Mr. Burnham's observations of double stars with the great refractor. We trust that this is merely an oversight on Prof. Hough's part, and that nothing has occurred to put a stop to, or even to interrupt, Mr. Burnham's work, the value of which it is impossible to estimate too highly.

THE WEDGE PHOTOMETER.—Mr. H. M. Parkhurst, well known as an experienced observer of variable stars, has communicated to the 'Sidereal Messenger' (November, 1885) some interesting remarks on the influence of the illumination of the background on observations made with the wedge photometer. He supposes that a 12th magnitude star is just visible through the thin end of the wedge, while Polaris, a 2nd magnitude star, is just visible at the thick edge. In this case the apparent light must be reduced 10,000 times by the wedge between these two points, and the light of the sky must be reduced in the same proportion. If now, in consequence of a bright Moon or twilight, the light of the sky is sufficient, even at the thick end of the wedge, to produce an extinguishing effect of ·01 magnitude, then at the thin end, where the illumination is 10,000 times greater, the effect must be very considerable, perhaps exceeding 1·0 magnitude. It being assumed that the scale of the wedge was determined with the sky dark, there will evidently be introduced a very appreciable difference of scale in the case of the bright background here considered. Mr. Parkhurst understands Prof. Pritchard to claim that the constancy of his observations of Polaris proves that if there is any error at all arising from this cause it is insensible. But he points out that Prof. Pritchard's observations merely show that the effect of the moonlight in the thick part of the wedge, where the light of a 2nd magnitude star is reduced to invisibility, is inappreciable. This, however, is just where we should expect the effect to be least; it is where the effect of the moonlight is reduced 100 times less than its effect in observing a 7th magnitude star through a thinner part of the wedge. In making some experiments bearing upon this point Mr. Parkhurst compared together the light of two stars of such relative brightness that in observing with a dark sky, the brighter through a shade and the fainter without it, the magnitudes were almost precisely equal. When the Moon's light was ·25, the brighter star seemed about as before, but the other had become too faint to measure. The brighter star, seen through the shade, was then compared with a third star seen without it; and allowing for the known difference in the magnitudes of these two comparison stars, it appeared that the fainter star was extinguished 1·2 magnitudes more than the star seen through the shade. This difference of 1·2 magnitudes caused by the moonlight corresponds to the error which would be produced by the same amount of moonlight in comparing the same stars with the wedge, the scale having been determined in a dark sky.

A PRINTING CHRONOGRAPH.—In his Report for the year ending May 20, 1885, Prof. Hough states that some time since he began the construction of a printing chronograph which has recently been put to actual work, the object being to obtain at once a printed record of the time of each observation.

"The apparatus consists of:—

"1. A system of clockwork carrying a type-wheel with fifty numbers on its rim, revolving once every second; one, two, or parts of two numbers being always printed, so that hundredths of seconds may be indicated. This train is primarily regulated to move uniformly by the Frauenhaufer friction balls, and secondarily by an electromagnet acting on the type-wheel and controlled by the standard clock. This train is entirely independent, and can be stopped at pleasure without interfering with the other type-wheels.

"2. A system of clockwork, consisting of two or more shafts carrying the type-wheels indicating the minutes and seconds. The motion of this train is also governed by an electro-magnet, controlled by the standard clock, operating an escapement in a manner analogous to the action of an ordinary clock, every motion of the escapement advancing the type one number.

"There are three type-wheels indicating minutes, seconds, and hundredths of seconds. The integer seconds are advanced at every oscillation of the standard pendulum, and the minute at the end of each complete revolution of the seconds wheel. The types were engraved on solid brass disks, which were previously spaced on a gear cutter. In front of the type-wheels is placed an electro-magnet of one ohm resistance, the armature of which carries three flexible arms, to which the printing hammers are attached. Under the printing hammer is a spool of paper two inches in width. One end of the paper is carried over the tops of the types and passed between two brass rollers. In the rear of the type-wheels small wooden rollers covered with cloth rest lightly against their rims for the purpose of inking the type. The type-wheels are readily set to indicate the time of the standard sidereal clock. The apparatus may be put in operation for recording observations in two or three minutes, including the preparation of the inking rollers and the setting of the type-wheels. The impression is made by means of the electro-magnet without stopping any of the type-wheels. The records therefore may follow each other as rapidly as the electro-magnet can be operated. The battery employed for printing consists of three small storage cells, equivalent to the same number of Grove elements. These cells are charged by means of a small gravity battery of eight elements, the whole arranged in series, so that connections need not be disturbed.

"It may be added that this is the only printing chronograph in use."

We have to record the death of Mr. John Hartnup, F.R.A.S., for many years Superintendent of the Bidston Observatory. The deceased received his astronomical training under the late Lord Wrottesley, whose assistant he was for some time at the Observatory on Blackheath. He was appointed Assistant-Secretary of the Royal Astronomical Society in 1838, and subsequently became Superintendent of the Liverpool Observatory at its foundation.

For many years Mr. Hartnup was an active observer of minor planets, comets, &c. with the refractor of the Observatory. Advancing years, however, compelled him to curtail his labours, and for some time past this excellent instrument appears to have been lying idle. Mr. Hartnup devoted himself with great success to the problem of determining the sea-going rate of a chronometer, the care and rating of chronometers belonging to the Mercantile Marine being the primary charge of his Observatory. He is also well known as the inventor of a form of compensation balance.

NOTICE TO SUBSCRIBERS.

Several foreign Subscribers having complained that the 'Observatory' reached them too late for the monthly Ephemerides to be of service, the Editors, in the July number of the present year, gave these for a month in advance, and have continued to do so since. It has, however, been pointed out that it would be much more convenient for observers were these particulars furnished in one collected form for the entire year, as at present it is often necessary to refer to two or even three different numbers to arrive at the information requisite for the work of a single evening. The Editors have therefore resolved to issue an extra number for the coming year, as a "Companion" to the 'Observatory,' to contain only Ephemerides and such astronomical information as may be useful to observers. The yearly volume will therefore in future contain thirteen numbers—the first number beginning the "Companion," which will be issued about a fortnight before the commencement of the year, and the other twelve appearing as usual at the commencement of the successive months. The ordinary numbers will contain, as before, a minimum of 32 pages, and will be entirely devoted to literary matter. The Annual Subscription will be *thirteen shillings* for the United Kingdom, *fourteen shillings* for countries included in the Postal Union, and *fifteen shillings* for all other foreign countries.

The "Companion" will contain the rising and setting of the Sun and Moon, ephemerides of variable stars, of the planets, of all their satellites, and for physical observations of the Sun, Jupiter, and Mars; the dates and radiant-points of the principal meteor-showers, catalogues of variable stars and various interesting objects, and of the more important nebulæ, binaries, and stars with unusual spectra; and other useful astronomical information.

The Titlepage and Index to the present volume will be issued with the "Companion," No. 105; but they will be sent separately, post-free, to any Subscriber for the whole of the present year, on application to the *Editors*, Royal Observatory, Greenwich.

ERRATA IN No. 103.

Page 391, footnote, *for* 262–63 *read* 2662–63.
392, line 4 from bottom, *for* 84·57 *read* 64·57.

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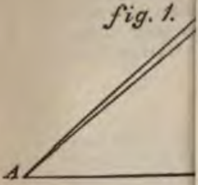

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From

Editor's Portfolio, January, 18

I HAVE received the following letter fro
Harris, whose geometrical work was
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"In the first place, I have to thank y
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commendations which I feel sure will pro
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teristics of the work to be beyond the
THE SCIENCE MONTHLY, and consequen
them from his review. Moreover, I m
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a copy. Moreover, I make the offer, if you are willing to allow me a page or a page and a half in your next edition, to send you a copy of the proposition referred to, with the woodcut of the diagram belonging to it, for the purpose of its insertion in your columns, so that your readers will then have the simple case directly before them without expense. The proposition is considerably under the average length of Euclid's; it is concise, and, although I believe its importance can scarcely be over-stated, it is neither abstruse nor difficult to understand.

"So then, if orthodox mathematicians throughout the length and breadth of the land are not *afraid*, that is to say, if they are not so nervous with regard to the *reality* of their precious and much-prized dogmas as to shrink from the encounter, let us try conclusions; and let some of them show (if they can), by the strict and well-known rules of geometry, that I am in the wrong. To call me or any one else 'a notorious circle-squarer,' is (to borrow the words of a well-known member of Parliament on a recent occasion) 'a strong mode of expression, but is not argument.'"

I fear it would not serve any good purpose to give Mr. Harris the space he asks. If he wants his proposition discussed by those who are best able to discuss it, he should submit a paper to the Mathematical Society.

February, 1885.

MR. JOHN HARRIS writes:—"To my letter published in the January number of THE SCIENCE MONTHLY you append the suggestion, that I should submit the matter for discussion to the Mathematical Society. The suggestion is a very natural one, and serves to exemplify how little, even by those best informed as to most matters in which general science is deeply concerned, the mediæval policy and intolerant attitude of technical mathematicians at the present time, with regard to certain questions, is appreciated or even known. I have been long since *officially* informed by the secretary of the Mathematical Society that on the three subjects, 'Duplication of the Cube,' 'Trisection of the Angle,' and 'Quadrature of the Circle,' no paper will be admitted, and no discussion whatever be allowed. More than this, the society not only *will not*, but, as matters now stand, it *can not* allow any, for it has taken the pains to debar itself from freedom of action in the matter by making it a fundamental rule of the society's constitution that no sort of discussion on any of these subjects shall be permitted. The position, virtually and in effect, is this: the Mathematical Society says, We have shut the door conclusively on these questions, and it shall not be re-opened; if any one make the attempt to open it we will prevent him, and he will incur the penalty of our severe displeasure. Very likely the existence of such a state of things in high places at the latter part of the nineteenth century will seem to most of your readers almost incredible; particular inquiry, [redacted] wever, will prove the statement to be stric te. These three subjects, be it observed, ne three of those which

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